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EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

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OF THE

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IN

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MEDICAL AND SURGICAL JOURNAL.

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*** The passage referred to on the phosphorescence of the glow-worm, in page 221 of this Number, will appear in the Medical Intelligence of our next.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr RITCHIE, Dr STRATTON, Mr T. D., and Mr W. MARTIN.

The following Publications have been received:—

Electricity as a Cause of Cholera, or other Epidemics, and the Relation of Galvanism to the Action of Remedies. By Sir James Murray, M. D., T. C. D. et Edin., M. R. C. S. Edin. Dublin, 1849. 12mo, Pp. 160.

Malignant Cholera: its Mode of Propagation and its Prevention By William Budd, M. D., Physician to the Bristol Infirmary. London. 1849. 8vo. Pp. 30.

Clinical Lectures on the Physical Diagnosis of Phthisis, Delivered at the Hospital for Consumption and Diseases of the Chest. By Richard Payne Cotton, M. D., Member of the R. C. of Physicians, London, Assistant Physician to the Hospital (From the London Medical Gazette). London, 1849. 8vo. Pp. 23.

Medical Ethics; or a Code of Institutes and Precepts Adapted to the Professional Conduct of Physicians and Surgeons. By the late Thomas Percival, M. D., F. R. S. Third Edition. Oxford, 1849. 12mo. Pp. 194.

Code of Ethics of the American Medical Association (reprinted from the American Edition). Oxford, 1849. 12mo. Pp. 36.

Duties and Qualifications of Physicians: an Introductory Lecture. By John Ware, M. D., Hersey Professor of the Theory and Practice of Physic in the University of Cambridge, United States. 12mo. Pp. 37.

Lectures on Electricity and Galvanism, in their Physiological and Therapeutical Relations, Delivered at the Royal College of Physicians. Revised and Extended. By Golding Bird, A. M., M. D., F. R. S., F. L. S. Fellow of the College, late President of the Westminster Medical Society, &c.; Assistant Physician to, and Professor of Materia Medica at, Guy's Hospital. London, 1849. 12mo. Pp. 212.

The Physiognomy of Diseases. By George Corfe, M. D.; Resident Medical Officer at the Middlesex Hospital, &c. &c. London, 1849. 4to. Pp. 151.

Quarterly Return of the Marriages, Births, and Deaths, registered in the Divisions, Counties, and Districts of England. Published by authority of the Registrar-General. No. 3, 1849. 8vo. Pp. 84.

On the Pathology and Mode of Communication of Cholera. By John Snow, M.D. (From the London Medical Gazette.) London, November 1849. 8vo. Pp. 15.

Contributions to the Pathology and Treatment of Eruptive Diseases of the Skin. By J. Moore Neligan, M. D., M. R. I. A., Physician to the Jervis Street Hospital, and Lecturer on the Practice of Medicine in the Dublin School of Medicine. Dublin. 8vo. Pp. 15.

The Physiology of Digestion, considered with relation to the Principles of Dietetics. By Andrew Combe, M. D., Fellow of the R. College of Physicians, Edinburgh, &c. Ninth Edition. Edited and adapted to the present state of Physiological and Chemical Science, by James Coxe, M.D., Fellow of the R. C. of Physicians, Edinburgh. Edinburgh, 1849. 8vo. Pp. 191.

The First Medical Report of the Hospital for Consumption and Diseases of the Chest, presented to the Committee of Management, by the Physicians of the Institution. London, 1849. 8vo. Pp. 42.

Memoir on the Ganglia and Nerves of the Uterus. By Robert Lee, M. D., F. R. S., Fellow of the R. College of Physicians, London; Physician to the British Lying-In Hospital; and Lecturer on Midwifery at St George's Hospital. With Five Engravings. London, 1849. 4to. Pp. 36.

On the Ganglia and Nerves of the Heart. By Robert Lee, M. D., F.R.S., &c. For the Philosophical Transaction. Part I., for 1849. London, 1849. 4to. Pp. 8.

On Tic Douloureux, and other painful Affections of the Nerves; with Suggestions for their Treatment by means of the Aneuralgicon. Illustrated by Numerous Cases, and an Engraving of the Apparatus. By C. Toogood Downing, M.D., F.R.C.S., Fellow of the London Medical and Westminster Societies; late Physician to the Blenheim Street Dispensary. London, 1849. Small 8vo. Pp. 73.

The Diseases of Children. By Fleetwood Churchill, M. D., M. R. I. A., Honorary Fellow of the King and Queen's College of Physicians in Ireland, &c. &c. Dublin, 1850. 12mo. Pp. 656.

On Stammering and its Treatment. By Bacc. Med. Oxon. London, 1850. 8vo. Pp. 64.

Sanitary Economics: or Our Medical Charities as they are, and as they ought to be. By Alexander P. Stewart, M. D., Physician to the St Pancras Royal General Dispensary. London, 1849. 8vo. Pp. 30.

Surgical Anatomy of the Arteries, and Descriptive Anatomy of the Heart. By the late Valentine Flood, M. D. A New Edition. By John Hatch Power, M. D., Fellow of the R. C. of Surgeons in Ireland; Surgeon to Jervis Street Hospital, &c. &c. Dublin, London, and Edinburgh 1850. 12mo. Pp. 189.

First Principles of Medicine. By Archibald Billing, M. D., A. M., F. R. S., Fellow of the R. Coll. of Physicians; Member of the Senate of the University of London, &c. &c. The Fifth Edition, Revised and Improved. London, 1849. 8vo. Pp. 332.

A Few Suggestions on Consumption. By Robert Hull, M. D., Extra-Licentiate of the R. Coll. of Physicians; Physician to the Norfolk and Norwich Hospital. London, 1849. 12mo. Pp. 138.

The London Medical Gazette, or Journal of Practical Medicine. No. 1138, N. S. 230, September 21, 1849,—to No. 1151, N. S. 243, December 21, 1849.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine and Surgery. No. 8. October, 1849.

London Journal of Medicine. No. 10, October; 11, November; 12, December, 1849.

The Dublin Quarterly Journal of Medical Science. No. 16. November, 1849.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. No. 8. October 1, 1849. London.

Weekly Tables of Mortality for the Metropolis. No. 38. 22d September 1849.—No. 50. 15th December 1849. Annual Series x.

The New York Journal of Medicine and the Collateral Sciences. September 1849. No. 38, New Series, Vol. iii., No. ii.

Communications, post-paid, to be addressed to Messrs Longman and Co., London; *or* Messrs Black and Co., Edinburgh.

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Page 221, 6th line from foot, *after* with, *insert* which.
225, 11th „ „ *for* seventeenth, *read* eighteenth.
229, 20th „ „ top, *for* nitrate, *read* nitre.
236, 6th „ „ foot, *for* nitrate, *read* citrate.
260, line 20, *delete* left.
„ „ 33, *for* repeated, *read* reported.
261, „ 2, *delete* membrane.
262, last line, *for* respiratory, *read* expiratory.
273, line 40, *for* congestion, *read* disease.
284, „ 26, *for* occasionally, *read* frequently.
290, „ 43, *for* lower, *read* inner.
314, „ 3, *for* as, *read* at.
316, „ 16, *for* inspiration, *read* expiration.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr RITCHIE, Dr B., and Mr LITTLE.

The following publications have been received:—

On the Operation for Strangulated Hernia. By Henry Hancock, F. R. C. S. E., President of the Medical Society, London, &c. &c. London, 1850. 8vo. Pp. 94.

Contributions to Mental Pathology, with Introductory Observations, containing the past and present state of the Insane in Ceylon, &c. By James George Davey, Licentiate of the R. College of Physicians, London, M. R. C. S. England, &c. London, 1850. Post 8vo. Pp. 276.

The Pathology of the Kidney in Scarlatina. Illustrated by Cases. By James Miller, M. D.. Extraordinary Member of the R. Medical Society of Edinburgh, Member of the R. College of Physicians, London, and Physician to the Western General Dispensary, Lisson Grove, &c. London, 1850. 8vo. Pp. 177.

Essays on Syphilis. Essay I. Syphilitic Sarcocele. By John Hamilton, Surgeon to the Richmond Hospital, Dublin. Dublin, 1849. 8vo. Pp. 40. Two Coloured Plates.

A Treatise on the Blue Epidemic Cholera. By Dr William L. M'Gregor, Surgeon, 1st Bengal European Fusileers. Delhi. No date. 8vo. Pp. 147.

The Cholera; What has it taught us? Its various modes of Treatment examined; and the Mode by Calomel shown to be the best. With full Report of numerous cases. By William J. Cox, M. R. C. S., L. S. A., M. C. S. London, 1850. 8vo. Pp. 26.

Statistics of Cholera by Assistant-Surgeon, Edward Balfour, of the Madras Army, in charge of the R. Hon. the Governor's Body Guard. Madras, 1849. 8vo. Pp. 70.

An Essay on Coral Reefs as the Cause of Blakang Mati Fever, and of the Fevers in Various Parts of the East. By Robert Little, Esq., Surgeon, Singapore. No date, but from internal evidence in 1849. 8vo. Pp. 111.

Some Account of the last Yellow Fever Epidemic of British Guiana. By Daniel Blair, M. D., Surgeon-General of British Guiana. Edited by John Davy, M. D., F. R. S., Lond., E., Inspector-General of Army Hospitals, &c. London, 1850. 8vo. Pp. 161.

The Hunterian Oration for 1850. By Frederick C. Skey, F. R. S., Member of the Council of the R. C. Surgeons, E., Professor of Descriptive Anatomy in the Medical College of St Bartholomew's Hospital, &c. &c. London, 1850. 8vo. Pp. 44.

Account of the History and Dissection of a Case of Malformation of the Urinary Bladder, with Remarks. By A. Melville M'Whinnie, F. R. C. S., &c. London, 1850. 8vo. Pp. 8.

An Inquiry how far Consumption is Curable; with Observations on the Treatment, and Cases Illustrating the Efficacy of Cod-Liver Oil. By James Turnbull, M. D., Physician to the Liverpool Infirmary. London. 8vo. Pp. 21.

Hastings considered as a Resort for Invalids; with Tables Illustrative of its Temperature, Salubrity, and the General Character of the Climate, showing its Suitability in Pulmonary and other Diseases; also Directions for the Choice of a Residence, and Hints as to Diet, Regimen, Bathing, &c. Second Edition. By James Mackness, M. D., Member of the Royal College of Physicians, &c. London, 1850. Post 8vo. Pp. 187.

Service Pipes for Water: An Investigation made at the Suggestion of the Board of Consulting Physicians of Boston. By E. N. Horsford Rumford, Professor in the University at Cambridge. Cambridge, 1849. United States. 8vo. Pp. 48.

Outlines of Medical Proof. Revised and Corrected, with Remarks on its Application to Certain Forms of Irregular Medicine. By Thomas Mayo, M. D., F. R. S., formerly Fellow of Oriel College, Oxford. London, 1850. 12mo. Pp. 84.

The Transactions of the American Medical Association, Instituted, 1847. Vol. II. Philadelphia, 1849. 8vo. Pp. 956.

The Accommodation of the Eye to Distances. By William Clay Wallace, M. D. New York, 1850. 8vo. Pp. 36.

A Treatise on the Climate and Meteorology of Madeira. By the late J. A. Mason, M. D., Inventor of Mason's Hygrometer. Edited by James Sheridan Knowles. To which are attached A Review of the State of Agriculture and of the Tenure of Land. By George Peacock, M. D., F. R. S., &c., and Dean of Ely and Lowndean, Professor of Astronomy in the University of Cambridge; and an Historical and Descriptive Account of the Island, and Guide to Visitors. By John Driver, Consul for Greece, Madeira, London. Liverpool, 1850. 8vo. Pp. 388.

The London Medical Gazette, or Journal of Practical Medicine. No. 1152, N. S. 244, December 28, 1849,—to 1163, N. S. 255, 15th March, 1850.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine. No. 9. January 1850.

The London Journal of Medicine. No. 13, January; No. 14, February. 15th March 1850.

The Dublin Quarterly Journal of Medical Science. No. 17, February 1850.

Weekly Tables of Mortality for the Metropolis. No. 51, 22d December, 1849, and 52, 29th December, 1849. No. 1, 5th January, 1850, to No. 11, 16th March, 1850.

Quarterly Returns, 1 and 4; 2 and 3 not received.

The American Journal of the Medical Sciences. Edited by Isaac Hays, M. D. No. 37. N. Series. January 1850.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1st JANUARY 1850.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Clinical Contributions to the Pathology, Diagnosis, and Treatment of Certain Chronic Diseases of the Heart.* By Charles Ritchie, M.D., late Senior Physician to the Royal Infirmary, Glasgow. (Continued from Vol. lxxii. p. 339.)

In my former communication I spoke of two generic forms of chronic disease of the heart;—one, a lesion of the capacity of the organ, having its initiative in obstructed circulation of the capillary subdivisions of the pulmonary artery, which lesion, primarily in the right heart, gradually, by the pressure of this on the aorta, occasioned disease in the left heart, and eventually in most of the other organs; and, second, another form in which the lesion was one of the structure of the aorta, or of the left heart, in the first instance, but in which the right heart and systemic veins and viscera in other parts became diseased in a secondary way.

I proceeded to speak of the first of those forms under the designation of simple congestive, or dextral disease of the heart, and proposed to consider it under the four divisions of its earlier, its transition, and its concluding stages, and its complications.

The early stage was alleged to be constituted by varying degrees of chronic bronchitis, emphysema of the lungs, and distensibility of the right heart, which latter terminated more or less rapidly according to the frequency and severity of the bronchial congestion, in a certain amount of permanent dilatation of the cavities and openings of the right heart. In the present communication, I would offer some observations on the mechanism, symptoms, and treatment of

B, Simple congestive or dextral disease of the heart in its transition stage. And first, in reference to the mechanism or mode by which this transition stage is formed, I propose to inquire what are the probable changes which an habitually distended and overloaded right heart will either immediately or prospectively occasion on the left side of the organ.

On looking at a normal heart injected, in reference to this point, it will be perceived that there is,—

1*st*, Pressure exerted by the column of blood passing along the upper part of the right ventricle, and the pulmonary artery, on the septum of the heart.

2*d*, The pressure of the right and posterior aspects of the pulmonary artery on the left side of the root of the aorta.

3*d*, The pressure of the upper part of the right ventricle on the anterior aspect of the aorta.

4*th*, The pressure of the right auricle on the right side of the aorta.

5*th*, Pressure of the right branch of the pulmonary artery on the aorta.

In a healthy heart the surfaces now referred to are so many points rather of contact than of pressure; but, in proportion as the balance of the circulation is disturbed, and the elasticity of the right heart is diminished by habitual engorgement, the principle of gravitation exerts less or more of its natural influence. And, accordingly, on examining such a heart distended by blood or injection, it will be found that, besides that there is a certain amount of pressure maintained on the septum, the exterior of the root of the aorta is strongly indented by the united weight on it of the upper portions of the overloaded right auricle and ventricle, and of the whole of the dilated pulmonary artery, while the circumference of the interior of its orifice is diminished more or less below the average.

When dextral or simple congestive disease has run its entire course, it is, during life, distinguished, in addition to an assemblage of rational signs, by the presence of a double bellows murmur sound over the left heart; and after death it is always found that, united with the pathological changes in the right side of the heart, there are hypertrophy, and still more marked dilatation of the left side, along with contraction in the aortic, and dilatation in the mitral orifice,—the structure of the valves and of the endocardium, at the same time, remaining healthy.

It is inconceivable that such radical alterations can be accomplished at once, or otherwise than in a very gradual and progressive way. And, accordingly, there is for common a comparatively slow and stealthy aggravation of all the symptoms in such patients, spread sometimes over a period of years, and having interwoven with it the course and history of repeated acute attacks. In many, during some such attack, the transition from the first to the second stage of the disease may be demonstrated as taking place under our immediate and actual observation, at once by the augmenting pulmonic and commencing systemic engorgement, and by the sudden occurrence of a systolic murmur in the left heart, or of a marked irregularity or intermission in the arterial pulse. In certain instances, also, instead of a murmur, the natural systolic sound will be found at first only stronger, rougher, more prolonged, and more deeply toned than before; and in not a few such patients this description of first sound is converted, on the occurrence of any cause which hurries the circulation, and, of course, which increases the obstruction, into a harsh, grating, and often into a perfectly-formed bellows murmur; and, in some again, this abnormal preponderance of the first sound is at its commencement observed only at the left nipple; while, at the right, it is the second sound which still remains most conspicuous;—thus showing a relation in the progression of the symptoms, with the amount of obstacle in the aortic orifice.

It seems reasonable to believe that the precise character of the transformations which are wrought on the left heart in this disease will receive modification in different individuals, according to incidental circumstances, as the greater or less resisting power of the constitution of the patient, and also, perhaps, the varying point of chief pressure of the one heart on the other. Thus, in some, at a certain but indefinite period after the commencement of the symptoms of lesion of the bronchial vessels, and of the right heart, in which the early stage of the disease consists, there is the sudden accession, in the course of some relapse, of palpitation of the heart and throbbing of the carotids, neither of which are normally essential to any stage of the affection; and in some such early cases, which prove fatal, the left ventricle is observed to be hypertrophied only, without any dilatation. It has seemed to me that the first effect of the pressure of the right heart in those individuals is the excitement of resistive efforts in the ventricle, and the consequent growth of its walls. Again, in others, and those also in whom there had for some time obtained a strongly marked lengthening of the second sound of the heart over the aortic orifice, a sudden accession of irregular pulse, such as is the token of a want of uniformity in the supply of blood by the mitral opening, is the prominent new feature which the case assumes; and I have thought it possible that with such, the yielding of the fibres of the left ventricle had been more remarkable than their resistance, and that simple dilatation of the ventricle and of its auricular opening were therefore among the earliest of the changes being accomplished. This inference is sanctioned by the many examples in this affection of the left ventricle being converted into a flaccid bag, and its auricular communication enlarged to twice its proper size, which are met with on dissection; and also by the fact, that in some of the patients at this stage, in whom a prolongation of the second sound of the heart is the chief physical sign observed at the aortic orifice, when the circulation is quiet, great intermission of the pulse arises when this is disturbed.

Again, the production of both the kinds of transformation of the ventricle to which reference has now been made,—of thickening and of dilatation,—appears often to be intimated by the symptoms during life, the murmur, to whatever extent developed, being in one series of cases heard most intense over the aortic orifice;—as if in such the ventricle had still its energies aroused to overcome the superincumbent pressure; and, in another, the left ventricular foramen being the centre of the systolic sound, as if, in that series, the resistance at the aorta was either positively or relatively so great, that the stream was being directed backward on the auricle.

Lastly, in other instances, the conditions here stated are observed to alternate; the large vessels at one time, and the left auricle at another, being the centre of the murmur, at different times, in the same patient; and to this corresponds the most frequent appearance of the parts after death, namely, dilatation, with some thickening of the ventricular walls.

In tracing the changes in the structure of the left heart now referred to, I may be permitted to mention that, in respect to the aorta, my habit was to pass the stethoscope along from over the origin of the vessel at the sternal edge of the third left intercostal space across to the right side of the sternum as high as the second rib; and the innominata was examined under the proximal end of the left clavicle. In this affection I rarely followed the aorta along the left side of the dorsal spine; but when, as often happened, its sounds or those of the *innominata* were masked by bronchitic noises, or by the interposition of emphysematous lung, the carotids supplied the information which was required. Most usually both cardiac sounds, but certainly the morbid one, will be heard with distinctness in the neck. And the fact of the repetition in that situation of pure aortic pathological sounds, affords us not only the means of distinguishing these from such as are either mitral or pericarditic, and of detecting them, when, although fully developed, they are yet obscured in the præcordia by bronchitic rhonchi, or pericardial effusion, but also, of establishing a correct diagnosis in those cases in which, from the feebleness of the murmurs, they are not well heard over the site of the left ventricle. In many patients the degree of departure in either sound from the standard of health, is for a long time inconsiderable, and the depth of the aortic orifice from the surface, or some other cause, may in such individuals prevent a ready recognition of anything in the physical signs which is abnormal. Now, I presume, it is on the obvious principle that the loudest noise is the farthest carried, that in such case the disordered aortic sound, whatever it may be, will be the chief stethoscopic phenomenon in the neck. At the heart it may not be easily distinguished from its accompanying healthy sound, or from itself in a state of absolute integrity, but at the neck it stands out prominently distinct from both.

In reference to mitral sounds, I found it to be borne out by observation, that, if the sounds were heard with most distinctness along a space bounded by the upper edge of the fourth rib, the pectoral edge of the axilla, and along the outer and inferior aspects of the nipple of the left side, then the main seat of the disease was in the corresponding auriculo-ventricular foramen. The evidence was complete, when the murmur so heard was diastolic, that emitted from other portions of the heart being systolic; or if, when itself systolic, it could yet be separated by the ear from the true systole of the heart by a sensible but most minute line of interval, forming with it something like a parallel or double systolic sound; or, lastly, when merged in and continuous with the natural ventricular sound, nothing corresponding to the murmur could be discerned in the aorta.

With these observations premised, I shall now detail some examples of what I esteem the transition stage of dextral or simple congestive disease of the heart. And first, I shall adduce some examples of the state under consideration in which the symptoms pursued the course that is most common and usual.

Case 8.—*Bronchitic and emphysematous lungs; prolonged second cardiac sound, and striking symptoms of pulmonary congestion, succeeded by aggravation of the symptoms, a mitral systolic sound, and death. Example of regurgitation along pulmonary artery, and afterwards by aorta, and by mitral valves.*

Elizabeth Morrison, aged 40, unmarried, a hawker, admitted January 15, 1847. This woman has had short cough for many years, and dyspnœa for twelve months. Eight days since, after exposure to cold, she became affected with pain along the right side, from the spine of ilium to shoulder. She is pigeon-breasted; the thorax is emphysematous, and precordial dulness lost. Over anterior region of the thorax the respiratory murmur is unusually loud; beneath clavicles the breathing is bronchial, and accompanied by rough crepitation. Posteriorly the murmur is generally sonorous or rough. Resonance of voice much increased at apices of both lungs, but especially at inner end of left clavicle. The second heart sound is prolonged and rough. The pulse is 84; appetite good; tongue white; bowels regular; menses natural. Pain of side is excited by motion or sudden presure, but relieved by steady support, so that she prefers to lie on the pained side.

Three grains of calomel, two grains of opium in powder, one grain of potassio-tartrate of antimony, and one scruple of extract of hyoscyamus, were directed to be formed into twelve pills, one of which to be taken every six hours. Sinapism to breast.

On the 17th, 12 grains of powder of squill, one drachm nitrate of potash, and two drachms bitartrate of potash, were ordered to be mixed and divided into twelve papers, one of which to be taken every four hours.

18*th*, Half a pill to be taken every four hours.

20*th*, Second cardiac sound heard prolonged and harsh over the site of the aorta, and is the only sound audible in neck; face leaden-coloured; inspirations 38; vesicular murmur absent throughout; respiration subcrepitant in lower, and sibilant or sonorous in upper parts; liver congested; belly tympanitic and obscurely fluctuant; frequent dyspnœa with syncope in erect position.

To take, every second hour, half an ounce of a mixture, consisting of one drachm of carbonate of ammonia, and one ounce of spirit of nitrous ether in six ounces of water.

22*d*, Lividity of lips; loss of vision in an erect posture; dyspnœa; copious viscid expectoration; meteorism of bowels; palpitation of heart; pulse 96; pulsations in neck single; mouth sore.

Omit the pills; apply blisters in succession to different parts of thorax. To have two ounces of gin daily; continue the rest.

Feb. 1*st*, Face continues bloated and livid; a systolic murmur well heard around whole of outer segment of left mamma, and also over sternum, but not in situation of aortic valves, has appeared.

The gin to be omitted, the other remedies to be continued.

5*th*, Pulsations of heart feeble and without sound; pulse imperceptible at wrist; appears nearly moribund.

To have half an ounce of gin every hour till she rallies.

7*th*, Patient's heat became restored after last report; but not having been seen in the interval, the gin was persevered in, and symptoms of congestion of the brain took place, for which a blister was applied, and her appearance to-day is improved.

To have one scruple of nitrate of potash, largely diluted with water, every four hours. Gin to be diminished to four ounces daily.

8*th*, Death at six P.M.

Inspection—Pleuræ free from adhesion. A moderate quantity of serum in each sac. Lungs emphysematous, not collapsing on exposure, and presenting some inflated vesicles on their surface. Tissue crepitant throughout, except at dependent parts, where there is much œdema. Bronchial lining reddened, and covered with thick mucus, which is purulent in large tubes.

Heart dilated and hypertrophied; weight, fifteen ounces. The right auriculo-ventricular opening permits the transmission of four fingers with ease; the left is nearly as large. The valves healthy in structure; aortic and pulmonic valves also unaltered in structure, but admit of slow regurgitation of fluid; pericardium natural.

Liver small, rather pale, and more brittle than usual.

Spleen normal.

Left kidney weighed eight ounces and a half, and right seven ounces and a half, and both were in the first stage of granular degeneration.

Case 9.—*Emphysematous lungs; bronchitis after delivery; prolongation and harshness of second cardiac sound; a relapse and accession of a continuous mitral sound and of anasarca; recovery.*

Janet Allan, aged 38, a widow, admitted February 23, 1847, was confined six weeks ago, and having risen too early, she four weeks since became affected with cough and shortness of breath. These symptoms continue; they prevent her lying with her head low, and are most troublesome at night; denies having been previously subject to any chest complaint; but the chest is rounded, and its percussion clear. The sounds emitted by the heart are loud, the second has a harsh character, and the jugular veins are dilated; the breathing is everywhere loud anteriorly, and above it is quite bronchial. On right side in front there is also moist crepitation of various degrees of fineness; posteriorly there is frequent sonorous rattle. Much vocal resonance all round tops of lungs. Ankles and feet have been œdematous for a fortnight; urine in natural quantity, not albuminous; pulse 120, feeble; expectoration frothy, partly viscid; appetite impaired; bowels said to be regular; sleeps badly; skin sallow; considerable debility; mammæ distended.

Blood to be drawn from upper part of thorax by cupping to four ounces; to have sinapisms applied daily between the shoulders, and under clavicles; and to take twenty-five grains of ipecacuan, as an emetic. Potassio-tartrate of antimony, one grain; nitrate of potash, two drachms: mix, and form into twelve powders, one to be taken every three hours.

25th, Take of sulphuric ether, tincture of castor, of each, two drachms; mix; to have the half instantly, the other in three hours. To have two ounces of gin daily.

26th, Saccharated carbonate of iron, nitrate of potash, of each one drachm; mix, and divide into twelve papers; one to be taken three times daily; continue the others.

March 1st, Œdema of legs continues.

Omit the powder of tartrate of antimony and nitre; add to each of the powders of carbonate of iron, one grain of squill, and continue to use as before.

3d, Sulphate of magnesia, two ounces; nitrate of potash, bitartrate of potash, of each half an ounce; water, one pound and a half; dissolve; two ounces of the solution to be taken every four hours; omit the gin; continue the rest.

9th, To take half an ounce of cod liver oil morning and evening.

13th, Dismissed relieved.

April 7, 1847, Re-admitted; continued pretty well till twelve days ago, when, after exposure to cold and fatigue, feet again became swelled; œdema has since involved the whole of lower extremities, and there is also effusion into peritoneal sac; is still subject to nocturnal exacerbations of dyspnœa and palpitation, during which she cannot keep her head low; physical signs are as before, excepting that the harshness of second heart sound is more marked, particularly at middle of sternum; pulse 108, feeble; tongue fair; some thirst; appetite good; bowels said to be regular, and quantity of urine natural.

To have half a drachm of compound powder of jalap morning and evening; also, one grain of powder of squill, and five of nitre, and the same quantity of cream of tartar, every six hours.

8th, To have three ounces of gin.

9th, To have one twelfth of a grain of powder of elaterium added to each dose of compound jalap powder.

13th, Gin to be diminished to two ounces daily.

15th, A rough continuous murmur is heard around whole of left mamma, from third costal cartilage anteriorly, to pectoral edge of axilla, and increasing in the latter direction; over the *arteria innominata* a diastolic sound only is heard.

18th, Omit the gin, and continue every thing else.

23d, Omit all the medicine, and repeat the gin.

25th, Turpentine enema.

27th, Repeat the compound powder of jalap, with elaterium.

28th, Turpentine enema.

29th, Increased dyspnœa and anasarca.

Take of squill 12 grains; mercury with chalk, powder of ipecacuan, of each four grains; nitre, two drachms; mix, and divide into twelve papers, one of which to be taken every three hours.

May 4th, Omit the gin.

10th, To have three ounces of gin daily.

12th, Much increase of anasarca, with extended and tumultuous action of the heart. Pulse 120.

Omit the gin. To take three-fourths of a grain of calomel, and one-fourth of a grain of opium, every three hours; continue the diuretic powders, and give a drachm of nitrous ether every two or three hours.

17th, Kidneys have begun to act powerfully, and patient is quite relieved.

To take the calomel and opium every six hours only; to have two ounces of infusion of quassia three times daily; to continue the diuretic medicine.

26th, Dismissed relieved.

In both of the foregoing cases there was an initial condition of chronic catarrh, and of dilatation of the air-cells, succeeded immediately before admission by acute symptoms indicative of pulmonary congestion, bronchitis, and engorgement of the right heart. In both the most prominent physical sign at first was the prolongation and harshness of the diastolic sound of the heart, associated with feebleness of the radial pulse. The exhaustion of the vital powers,—as shown by these symptoms, and by the single diastolic heart sound heard in the neck and *arteria innominata*, the disposition to syncope, the meteorism of the bowels, and the apparent call for stimulus,—was in each also early noticed. In both patients, too, at a time distinguished by the arrest of the urinary and other secretions,—circumstances which strongly indicate the venous engorgement of the general viscera,—and by great lividity of the face, some previous palpitation, and much dyspnœa; when the left ventricle was receiving continuously, by the enlarged mitral opening, more than it could expel,—at the same moment that its own stream was returning on it from the aorta at every diastole,—and when, from increasing feebleness, it had also become less competent to overcome the aortic obstruction, a systolic murmur best heard in the vicinity of the mitral valve,—the external sign of regurgitation into the left auricle, became conspicuous. In one of the parties, the failure in the energy of the heart, as shown in the feebleness of its natural sounds, the extinction of the diseased murmur, and the loss of pulse was great. The congestion also of the kidneys had attained the maximum degree. These circumstances are adverse to the success of diuretics. In the other patient, who recovered, the structural changes in the heart,—namely, enlargement of its cavities and auriculo-ventricular openings,—may not have differed greatly from those in the patient that died; but the powers of life were in greater integrity, and the tubular structure of the kidneys perhaps less compressed, and every menacing symptom vanished instantly on the secretion of urine being restored.

Such are the most ordinary aspects which the disease under consideration progressively assumes. But I have said that there is an early period of this transition course in which, more remarkably in some constitutions than in others, the efforts of the left ventricle are strongly exerted towards the removal of the obstruction, and that, under such circumstances, thickening of the walls of that side of the heart is commonly induced.

The following appear to be examples of this variety.

Case 10.—*Chronic bronchitis, paroxysmal dyspnœa, palpitations, cerebral congestion, spasmodic twitches, anasarca, stupor, death; dilatation of right, and hypertrophy of left ventricle.*

Henry O'Neil, aged 29, a weaver, admitted 9th January 1830, complains of severe cough, and scanty expectoration of yellow matter, attended with a sense of oppression at the *scrobiculus cordis.* Cough is most severe during night. Distressing dyspnœa occurs in paroxysms at irregular intervals, but chiefly during night, when, awaking suddenly from sleep, he feels a distressing tightness about his chest, as if he were bound with cords. At the beginning of the paroxysms he has a harsh, dry cough, which, lasting for some time, an expectoration of tough, viscid mucus commences, which gradually becomes copious, and affords relief. Respiration is much affected by the state of the atmosphere. He cannot assume the horizontal position. He has palpitation in region of heart, and a feeling as if something were ascending from that to the head, at which time, also, he becomes confused, and has vertigo and *tinnitus aurium.* Sleeps ill; appetite deficient; tongue foul; bowels costive; pulse 84; skin natural. For about the last three years has been subject to cough and dyspnœa, which of late have become aggravated.

To have one of ounce castor oil.

10*th*, As at report.

A blister to be applied over the breast; and an issue afterwards to be established. A cough mixture consisting of mucilage with laudanum and tincture of squill.

13*th*, Cough and dyspnœa aggravated. Face anxious and colour dusky. Pain of side.

Twelve leeches to be applied over the seat of the pain.

15*th*, Pain of side abated. Dyspnœa continues distressing at night.

Three grains of sulphate of quinine and two grains of opium to be made into a pill with syrup of squill, and given at the hour of composing to rest.

16*th*, Spasmodic twitches all over his body. Is occasionally incoherent. Complains of beating within the head.

The hair to be removed, and a blister applied to the scalp.

17*th*, Few spasmodic twitches; dyspnœa moderate; anasarca of limbs.

He was directed to take three times daily half an ounce of a

mixture consisting of eight ounces of infusion of foxglove and one ounce of tincture of squill.

18th, Still more incoherent; pulse feeble. Takes no food.

To have one ounce of gin in water every second hour. Allowed eight ounces.

19th, Urine one pound. Three stools from salts.

Death on the 21st.

Inspection.—Bronchial tubes much inflamed, especially at the principal divisions, and in some places their mucous membrane was thickened. Heart was much dilated, particularly the right ventricle. Left ventricle was hypertrophied; the valves were natural.

In the pericardium were about eight ounces of fluid, and in the pleuræ four.

In this case there was initiatory bronchitis and dyspnœa of three years' duration. Where an intelligent care to avoid exposure to cold is observed by the patient, or in cases in which the pulmonary congestion is kept in abeyance by an habitual copious expectoration, the occurrence of disease in the left heart is often protracted to a very long period. But in labourers out of doors, and the working class generally, the whole duration often of the fully perfected disease does not occupy more than three or four years.

The sense of girding and oppression at the ensiform cartilage, of which O'Neil complained, is a common symptom, and has appeared to me to arise sometimes from the violent action of the diaphragm, and at others from the engorgement of the heart, or of the liver, or of the pressure of the former on the latter. Whatever truth may be in such conjectures, the symptom is always accompanied by augmented movements of the diaphragm, which in process of time occasion a deep persistent depression of the ensiform cartilage and lower end of the sternum, which remains a visible and strongly-marked sign of the laborious efforts, with which the breathing in such subjects has been habitually carried on.

The increase of cough and dyspnœa during the night are also very uniform symptoms; and appear to me to depend in a great measure on the relative tardiness of the heart's pulsations during the night as compared with the day. The circumstance of position does not explain it, as, in patients who cannot recline, but require to be retained in a sitting posture constantly, the nocturnal aggravation of the dyspnœa and cough is often remarkable. The greater warmth, and consequent rarity, of the atmosphere, and diminution in the quantity of its oxygen, as well as its greater mechanical mixture with impurities in the night than in the day, may have something to do with these symptoms; yet I believe the greater pulmonary plethora which arises in a state of mental and physical quiescence is their more frequent cause. The aggregate number of pulsations at the wrist in certain observations made by Mr Grieve, one of my clerks, on some cardiac cases, was for the day 7682, and, in the same patients for the night, it was no more than 7347, giving an excess of 335 contractions of the heart during the excitement of the day as compared with the night. In whatever way we explain this fact, it is of use to remember it in giving opium or digitalis to such patients. If seen during even an ordinary sleep, the hue of the countenance will be observed to have become darker, and in some cases the extreme parts, as the tips of the nose and ears, are found deeply livid. The irritability of the heart when thus diseased is easily exhausted, and the patient quieted with even a moderate dose of opium, may have the action of this organ fatally arrested during sleep; or, in the case of digitalis, syncope may terminate his existence in the midst of some very moderate muscular effort.

In another individual, the subject of the next case, the hypertrophy of the left ventricle, as in the cases just quoted, was very considerable, and the palpitation and precordial dulness were also greatly more striking than is usual in simple congestive cardiac disease; and, in such circumstances, we will be more easily induced to hazard the employment of sedative treatment. The case proves, that it is not always safe to employ digitalis, however, simply because there are symptoms present of apparent high activity of the heart.

Case 11.—*Great dyspnœa, lividity of countenance, and anasarca; right heart much dilated, and left greatly hypertrophied; valves sound.*

John Duvennay, aged 37, a labourer, admitted on the 12th January 1837. This man complains of urgent dyspnœa, in general preventing him from assuming the horizontal posture; severe cough, with copious frothy mucous expectoration. He has pain and much uneasiness in the region of the heart, and palpitation, which is more or less severe according to the exertion he makes. An evident deformity of the chest is observed; the lower left ribs protruding much more than the right. Slight precordial dulness, and increased and diffused action of heart, are also recognised; sounds clear and distinct. Anasarca of lower limbs and of the chest. The face is swollen, and the cheeks and lips are very livid. The abdomen is distended, but does not fluctuate very distinctly.

The general health is much impaired; little appetite; some thirst; sleeps badly. The patient inclines to lie on the left side. Pulse 92 when in bed, and pretty regular. Tongue thickly furred, but moist. Makes little water; has uneasiness in lumbar region, somewhat increased by pressure. He was a patient in this hospital about nine years since with a similar affection, and when dismissed was greatly relieved. He remained in tolerable health till about eighteen months since, when the dyspnœa and palpitation again attacked him. Dropsical swelling supervened six weeks ago. Has had no particular treatment for some time.

To have an anodyne draught.

13th, Pulse after he has been in the erect position about 110. In other respects as at report.

To take three times daily half an ounce of the infusion of foxglove; and every eight hours one pill consisting of one grain of calomel and one grain of squill.

15th, Dyspnœa increased. Great lividity of face; pulse very feeble. Urine turbid and in greater quantity.—Continue the same remedies.

16th, Vomiting after a fit of coughing; slight stupor. Pulse scarcely perceptible.—Omit the remedies.

17th, Death at 4 A. M.

Inspection.—The pericardium contained about four ounces of muddy serum, and there were several slight deposits of lymph on its inner surface. The heart was considerably enlarged; the right ventricle very much dilated, and its walls remarkably thin. The left ventricle was also enlarged, and its parietes were at least six times as thick as those of right. All the valves were perfectly sound.

Parenchyma of lungs engorged; redness and thickening of inner surface of bronchi.

Liver very firm.

The augmented and extended cardiac impulse noted in this man's case, combined with the dulness of the part on percussion, afforded good evidence of hypertrophy of the left ventricle; but the clearness and distinctness of the heart's sounds, and the extent of range in the radial pulse from 92 when the man reclined, to 110 when he stood, were equally conclusive proofs that an enlarged or dilated and enfeebled state of the cavities was the dominant condition. The use of sedatives in such circumstances is always hurtful.

It will be remarked that this man preferred to rest on his left side. This is the general rule in simple congestive cases; and *decubitus* on the back or right side can seldom be well borne. I have supposed that in sinistral *decubitus* not only was there no inconvenience felt from the approximation of the heart to the ribs, so considerable a cushion of emphysematous lung being generally interposed, but that in this position also, the egress of blood from the pulmonary artery was rendered more easy. On such a patient assuming the supine posture, the united pressure of the right heart and enlarged pulmonary artery bears immediately on the root of the aorta. Remarkable exceptions to the former of those general circumstances are, however, occasioned by accidental complications in the lungs. Thus acute pain in the left side, or severe bronchitis and copious effusion into the bronchia of the right lung, or into its pleura, may greatly modify the position.

Case 12.—*Chronic Bronchitis, palpitation, and anasarca; dilated heart, hypertrophy of left ventricle, valves sound.*

Widow Boag, aged 60, admitted February 10th, 1837. This woman has severe cough, rather copious, muco-purulent expectoration, and some uneasiness in right hypochondriac, and in both lumbar regions on pressure; sleeps ill; much thirst; general anasarca; urine scanty; general health impaired; pulse 84; tongue pretty clean; palpitation and dyspnœa on exertion. There is a strong impulsion at the lower part of the sternum, and in the præcordial region; and the sounds of heart can be heard nearly on all parts of thorax. A slight bellows murmur accompanies the second sound. Says that the dyspnœa has troubled her for about eighteen months, and that the palpitation and swelling only began about three weeks since.

13*th*, Symptoms rather aggravated, and pulse more feeble.

To take, at the hour of composing to rest, a draught, consisting of half a drachm of sulphuric ether, two drachms of camphorated tincture of opium, and one ounce and a half of peppermint water; and, next morning, half an ounce of bitartrate of potass (cream of tartar).

14*th*, Died.

Autopsy.—The heart was very much enlarged, and there was slight hypertrophy of the left ventricle; all the valves were sound.

Case 13.—*Bronchitis after delivery, dyspnœa, lividity, anasarca, and recovery; relapse and death; dilatation of right, and concentric hypertrophy of left heart.*

Marion Brown, aged 40, a weaver, married, admitted April 23, 1847. Two months ago this woman was delivered in the eighth month of pregnancy. The feet had been œdematous, and continued so after delivery. In a few days she was exposed to cold, and became affected with severe cough, which has continued since. Within the last two weeks anasarca has become general. The abdomen is large but soft, and not fluctuant. The whole of left side of thorax is dull on percussion, and, on applying the stethoscope, nothing but a very faint and distant respiratory sound is audible. On right side percussion is clear; respiration is harsh and sibilant. About middle of posterior aspect of this side also there is an occasional appearance of pleuritic friction sound. Has not had any pain here, or in any part of left side. Since the supervention of the general anasarca, patient has also suffered from violent dyspnœa. At present inspirations are 38. Decubitus on left side; face and lips livid. Pulse 104, feeble. No abnormal sound in heart's action, but there is much turgescence of the veins of the neck. Tongue dry; much thirst: appetite bad; bowels said to be regular; urine scanty. She denies having been subject to cough previous to present illness, or having been dropsical before.

To have every four hours half a drachm of the compound jalap powder, and the diuretic powder without digitalis.*

25th, Urine slightly albuminous.

To each dose of the compound jalap powder let there be added one-tenth of a grain of elaterium.

26th, To take one-fourth of the pill of calomel and opium every second hour;† and half an ounce of the mixture of carbonate of ammonia every third hour.‡

29th, A blister to be applied over the sternum, and the blistered surface to be dressed with mercurial ointment.

30th, To take, for alleviating the cough, the hospital pectoral mixture.§

May 1st, Mouth sore; pulsation in both jugulars; prolonged diastolic sound remarkable over right ventricle; occasional decubitus on right side. Urine four pounds. Swelling limited to feet and hands.

Pills of calomel and opium to be given up. Other remedies to be continued.

7th, Pills of calomel and opium to be resumed.

10th, Pills to be given up. Other remedies to be continued. A blister to be applied over chest.

28th, Dismissed relieved.

June 24th, Readmitted with much dyspnœa, general anasarca, and coldness of extremities. Pulse gone.

To have immediately six ounces of port wine.

25th, Dyspnœa continues, but she is otherwise better.

A blister to be applied over the region of the heart. To have every fourth hour the third part of a pill of calomel and opium. A turpentine enema to be administered.

26th, Pulse can hardly be counted, and is intermitting and irregular; tympanites less; respiratory murmur and sonorousness on percussion gone in whole of left side of thorax; gums pale; inspirations 48; no jugular pulse. Continue.

27th. Died last night.

Inspection on the 28th.—Body much emaciated. Right thorax contained about twenty ounces of serum; lung adhered to ribs posteriorly; tubes somewhat dilated. The left side of the thorax contained fifty ounces of dark-coloured serum. The pleura was thickened and rough from extensive deposits of lymph.

The pericardium contained ten ounces of serum. Heart of a brown colour, and soft. Left heart in a state of concentric hypertrophy. Right heart dilated; auriculo-ventricular opening admitted easily the passage of five fingers; walls of auricle diaphanous.

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* The formula for this powder in the Glasgow Royal Infirmary is,—Cream of tartar, one scruple; nitre, ten grains; powder of squill, one grain; powder of digitalis, half a grain. Mix.

† Each of these pills contains two grains of calomel, and nearly half a grain of opium.

‡ This mixture consists of one drachm of carbonate of ammonia dissolved in eight ounces of water.

§ The pectoral mixture is made of one drachm of the tincture of henbane and four ounces of mucilage.

It might have been supposed that the dulness on percussion over the left thorax in this patient arose from the œdema of the surface, produced by constant decubitus on that side, and also that the faintness of the respiration there was dependent on the emphysema, which might very reasonably be thought present in such a case. The inspection showed that a flake of lymph was the cause of both symptoms. The frequency of the inspirations,—in the first seizure 38 per minute only, and in the second 48,—is obviously a good deal correspondent to the degree of hindrance existing to the pulmonary circulation, and perhaps also in some measure proportioned to the smallness of the jet projected from the right ventricle. I found this last remark on the circumstance mentioned, that when the inspirations had risen to 48 per minute, the power of the right ventricle had become so enfeebled that the jugular pulse had ceased. The present case affords an easily intelligible explanation of this latter symptom. The right auricle and ventricle were substantially one cavity in consequence of the extreme enlargement of the tricuspid opening; and every contraction of the ventricle, while of ordinary strength, must have propelled a wave of blood into the auricle as certainly as into the pulmonary artery. The cessation of this symptom on the accession of great debility is an important fact. The force of the pulsations in the jugular veins in such patients may indeed be regarded as a fair measure of the tone and vigour of the heart, as the degree of venous pulsation varies with the state of the general strength. The rapid breathing, livid face, extreme thirst, feeble or lost pulse, and scanty secretion of urine, described in this woman, and which are often associated in similar cases with anxious, shrunk features, projecting eyes, wet skin and hair, and death-like coldness of the surface, sometimes forcibly recall the recollection of the collapse of Asiatic cholera; and the congestion in the right heart and general viscera in the two affections, present a pathological as well as a semiotic resemblance.

Case 14.—*Chronic bronchitis of ten years' standing; a wetting, followed by great pulmonary congestion, loud systolic sound of heart, palpitation, and death; dilated right heart, and hypertrophied left heart.*

Mary Corrigan, aged 22, unmarried, a mill-worker, admitted on the 2d November 1846. This patient had an attack of cold ten years ago, and often since has been more or less subject to cough and shortness of breath, especially during winter or close damp weather, but getting mitigated on the approach of summer. This summer the relief was less marked than usual, and the general health has been for long very indifferent. Five days ago, after getting wet, had a shivering, followed by aggravation of the cough and dyspnœa, with some headach and great debility. The symptoms have increased since; and for the last three days she has spit blood. Has frequent fainting fits: breathing very difficult; face livid; extremities cold; pulse 132, feeble; inspirations 40; breathing abdominal.

All over thorax the vesicular murmur is supplanted, both in inspiration and expiration, by sibilous and sonorous rattles, except below, where these are moist. The heart's action is tumultuous, with some preponderance of first sound. Sputa viscid and bloody. Never had any dropsical affection. Catamenia regular.

To have immediately two ounces of wine. To take of the mixture of carbonate of ammonia, half an ounce every second hour. To eight ounces of the hospital pectoral mixture, let there be added one grain of tartrate of antimony; and of this let the patient take half an ounce every second hour.

3d, Hæmoptysis continues.

Twelve grains of tartrate of antimony to be dissolved in six ounces of water, and one scruple of solution of muriate of morphia to be added. Of this one drachm to be taken every hour; and after the third dose two drachms. To use the catechu mixture, if requisite.

5th, Hæmoptysis ceased; sputum opaque and nummular; pulse 108.

To take of the opiate antimonial solution only one drachm every hour.

6th, To take half of a pill of calomel and opium every third hour. Other medicines to be continued.

7th, Much mucous rattle, with dyspnœa, and inability to retain horizontal position.

Antimonial solution and cough mixture to be given up. Continue pills and mixture of carbonate of ammonia.

8th, To have six ounces of port wine.

11*th*, Wine to be increased to ten ounces daily. To take, as soon as possible, one ounce of the mixture of carbonate of ammonia, with half a drachm of sulphuric ether. The feet to be placed in warm water, containing mustard.

11*th*, Death took place at 4 P. M.

Inspection.—Both pleural cavities obliterated by adhesions of some standing. Tissue of lungs engorged with serum; and, on making sections, purulent matter exudes from the bronchi. A small part of lower edge of right lung in a state of red hepatization.

The heart was hypertrophied; and left ventricle was also considerably dilated. The organ weighed ten ounces.

Liver of great size and weight, and containing much blood.

Case 15.—*Bronchitis of one year, and palpitation of ten days' standing; effusion into thorax, and death. Great congestion of thoracic and abdominal viscera, with dilatation of right, and hypertrophy of left, heart.*

Alexander Mackenzie, aged 49, a sawyer, admitted on the 6th October 1847. A year ago this man had bronchitis, from which he made a partial recovery in about six months; and two weeks after, had renewal of symptoms, increasing in severity to the present date. In the last ten days he has become affected with palpitation of heart, increased by any exertion. He has at present loose cough, epigastric pains; and for two weeks past has sweated much at night, and suffered from headach. Face slightly flushed; skin hot, and moist; pulse 68, full, and somewhat soft; bowels slow.

Extensive cardiac dulness; and a double murmur is heard over left side of heart. Breathing harsh and sonorous over anterior aspect of both, but especially of right lung; and posteriorly on this side there is some moist crepitus, and dulness on percussion. Limbs anasarcous. Belly tympanitic, slightly fluctuant, and very tender superiorly. Tongue moist, of a pink colour, slightly indented, and covered with a yellow fur. Gums spongy.

To have the third part of a calomel and opium pill every two hours; and half a drachm of compound powder of jalap morning and evening.

9th, Gums sore. Omit the pills.

11th, Urine continues scanty, of a deep orange colour. Specific gravity, 1·015, with acid reaction, and free from albumen.

To have a diuretic powder, without digitalis, every four hours, and the compound jalap powder to be continued night and morning.

15th, Over *arteria innominata*, and in situation of tricuspid valve, a single systolic blowing sound only is heard. At ascending aorta, middle of left ventricle, aortic valves, and edge of left pectoral muscle, in front of axilla, a soft full bellows murmur is heard with both sounds of heart, diminishing in fulness and intensity in the order in which the parts have been named.

To have five ounces of wine daily. Continue the diuretic and purgative powder.

17th, Some hemoptysis and epistaxis. A continuous or merged double murmur is heard to-day, both in neck and over *arteria innominata* and tricuspid valve. In other situations it is as at last report, excepting that it is stronger, although the patient complains of weakness and disposition to faint.

To omit the wine. To have half a drachm of taraxacum juice, and the same quantity of nitrous ether every six hours. A turpentine enema.

19th, Feelings more comfortable. A large full bellows murmur accompanies both cardiac sounds in every part of heart and large vessels, save in the neck, where the murmur is harsh and continuous. Kidneys have not begun to act.

Continue the diuretic powders. A blister to be applied to left side of thorax. To have the thighs rubbed every six hours with a drachm of mercurial ointment.

20th, Died.

Inspection thirty-six hours after death.—A little serum was seen flowing from the eyes, nose, and mouth. Numerous purple discolorations over whole surface. No pulmonary dulness, but that of præcordia occupied a space of nearly five square inches. Some varicose veins of large size over abdomen, and those of neck and arms also distended.

The thorax contained fully a gallon of bloody serum. The left lung was thrust into upper and posterior part of thorax by the heart, which also encroached somewhat on right. No fluid was contained within pericardium.

The heart lay transversely, and its long diameter measured *in situ* eight inches. Inclusive of pericardium, its weight was thirty-one ounces avoirdupois. The coronary veins were greatly distended. The walls of the left ventricle were about an inch in thickness, and its cavity somewhat enlarged. The aortic orifice could not admit the passage of a larger body than the full-sized middle finger; that of the pulmonary artery was fully twice as large. The right auriculo-ventricular opening permitted the passage of four, and that of left side of three fingers. A few warts on aortic valves; the structure of the latter was healthy.

The right lung weighed nineteen, and the left seventeen ounces. The bronchia appeared large, and their lining thickened.

Abdomen contained about three-fourths of a gallon of pure serum.

The liver was as heavy as five pounds five ounces. Large lobe very soft and pulpy; smaller, dark-coloured and granular.

The spleen was fully ten inches in length, and weighed twenty-nine ounces and a-half. It was semifluid, of a deep purple colour, having imbedded in it a couple of patches of fatty-looking matter.

The right kidney weighed six and one-fourth,—and left, six and one-half ounces,—and both were highly granulated, white, and pulpy, with here and there portions of a deep purple colour, interspersed with veins filled with red blood.

The modifications in the bellows sound, which were noticed in this patient under varying circumstances, are of some interest. The diastolic murmur was heard equally over the base of both ventricles; but in the aorta it extended a short way only on one occasion along the ascending vessel; and on another, when the strength was greater, it was observed as high as the carotid, but mixed or coalesced with the systolic bellows-sound. This continuous murmur produced by the coalition of the two sounds of the heart must be carefully distinguished from the single sound which is heard in cases of great debility of the organ. The latter consists of the second sound only, the former of both; and it is sometimes difficult to identify them. Thus, in certain cases of continuous murmur in weak nervous subjects, should the pulsations be rapid, the heart's sound is short, acute, and snapping, like what is heard in the collapse of typhus; and it is only after a few days' rest, and when the frequency of the heart's action is moderated, that the sound becomes louder, more sustained, and having either its rasping or blowing character developed. This circumstance depends on the fact, that in very high degrees of obstruction murmurs are annihilated; the increased velocity in the case in question being equivalent to such a degree of structural obstruction as is adequate to produce that effect. In other instances of continuous friction sound of the heart, the single lengthened murmur, after rest and other suitable treatment, is found broken down in certain parts of the organ, if not in all, into a double sound. This again, while it supplies us with a ready way of distinguishing a coalescent continuous cardiac sound from one that is strictly single,—which latter is usually though not universally the same in every part,—is a necessary effect of the composite structure of a continuous murmur, and a proof, also, that such murmur indicates a higher degree of lesion in the heart than is done by a double sound. In patients with the single sound of debility, a little excitement of the heart has sometimes the effect of restoring for a short time the systolic sound, which is that which is absent, while in certain cases of double cardiac sound the effect of a similar cause is to merge the two into a protracted continuous murmur.

The five cases last detailed are presented as examples of a frequent variety of the transition stage of simple congestive cardiac disease, in which the first indication of the involvement of the left ventricle is an increase of its action, attended by hypertrophy of its structure. In other cases the resistance of the systemic side of the heart to the pressure of the right heart on the aorta appears to give way at once, without any marked intervening tumultuary action, increased sound in the systole, or production of hypertrophy. A harsh prolonged diastolic sound, or sometimes a murmur, is heard during life in the aorta, accompanied sometimes by an intermittent, and in confirmed cases by a constant, systolic sound over the mitral orifice; and after death the left ventricle is found softened, thinned, enlarged, and flabby, and the mitral opening more or less dilated. The following may serve as an example.

Case 16.—*Catarrhal habit, thorax resonant; parturition and bronchitis. On admission, preponderance of diastole, enlarged jugular veins and liver; 8th day, dyspnœa and œdema of feet; 11th day, intermission of pulse; 20th day, jugular pulse; 25th, increased dyspnœa; 41st, increased anasarca; 48th, still greater anasarca; 53d, some improvement; 65th, relapse; 68th, orthopnœa, and abscess in abdominal parietes; 71st, erythema of leg; 75th, death—dilated cavities and foramina of heart, excepting aortic, which was contracted.*

Anne Mulroney, married, aged 25, admitted on March 20th, 1847. Nine days ago this woman was delivered of her third child. She is said to have been in good health previously, and the labour was easy. Lochia continued for two hours only, and have not reappeared. Six days ago, not having left her bed, became affected with cough, tightness at throat and upper part of chest, and dyspnœa, with fever. Was blooded at the arm on the same day, and blistered over the trachea on that following, with temporary relief. At present the symptoms are again very severe. Breathing hurried and wheezing; face pale; mucous membrane of mouth sublivid. Pulse 130, very compressible. Much general debility. Percussion of chest elicits a clear sound. Loud, sonorous, and sibilant *rhonchi* heard all over both lungs, but most intensely on left side. These sounds are accompanied by loud and harsh respiration. Sputum rather scanty, semipurulent, and very tenacious. Second heart sound preponderates; jugular veins dilated. Is subject to colds, but had not any cough immediately before her confinement. Lower extremities are somewhat œdematous; has much thirst, some appetite; bowels slow; tongue clean. Abdomen is full and soft; tender in epigastrium, but not at all over the uterus.

To take every third hour half of a calomel and opium pill.

21st, To have every third hour half an ounce of the mixture of carbonate of ammonia. Pills of calomel and opium to be continued.

22d, To take every sixth hour only half a calomel and opium pill.

23d, Pills to be given up.

The belly to be carefully fomented with cloths wrung out of hot water; to take as soon as possible half an ounce of oil of turpentine in mucilage; which is to be repeated at the end of four hours, unless the bowels have acted.

After the operation of the turpentine, to take one grain of soft opium every fourth hour, unless sleep be induced.

26th, Pain limited to-day to right hypochondrium, where liver is found much enlarged, and very sensitive. Pulse 120, and feeble.

Three or four ounces of blood to be drawn by cupping from the space below the right mamma.

A sinapism to be applied to the right hypochondre, and afterwards warm fomentations.

To have the fourth part of a pill of calomel and opium every second hour. Other remedies to be abandoned.

29*th*, Dyspnœa, œdema of feet, and tumour of liver, continue. Pulse 108.

To have every sixth hour a dose of the diuretic powder without digitalis.

Powder of ipecacuan, twelve grains; extract of chamomile, two scruples; extract of aconite, one scruple; to be mixed, and made into twelve pills, of which, half of a pill is to be taken every sixth hour. Pills of calomel and opium to be given up.

31*st*, Cannot retain horizontal position. Œdema over whole of lower limbs. Diastolic sound extremely prolonged and rough. Much dulness along whole of left inferior thorax, with bronchial breathing.

To have, as soon as may be, half a drachm of compound jalap powder, with the eighth part of one grain of elaterium, and let it be repeated every third hour, until the bowels are freely emptied.

In the evening, if breathlessness continues, three ounces of blood drawn by cupping from the left side of the chest, at the part most uneasy. To take a draught of one scruple of sulphuric ether, if requisite.

April 1*st*, Dyspnœa relieved; dulness of side continues, but without pneumonic crepitus. Pulse 96, soft and intermitting.

To have one ounce of the mixture of carbonate of ammonia every second hour; three ounces of port wine. Other remedies in the meantime to be given up.

8*th*, Wine to be reduced to two ounces.

Ten grains of saccharine carbonate of iron, and one grain and a-half of sulphate of quinine, to be made into a powder, to be taken morning and evening. Twelve powders to be prepared.

9*th*, Right jugular vein, which is much distended, the seat of pulsation throughout its course, arrested only by slight pressure at clavicle.

11*th*, Arrest of urinary secretion.

Medicines to be omitted. To take every fourth hour one diuretic powder without digitalis.

14*th*, Increase of dyspnœa; diastolic sound over aortic valves so prolonged and rough as to amount to an imperfect bruit.

To six drachms of the compound jalap powder, one grain of elaterium to be added, and the whole to be divided into twelve parts, one to be given every four hours, until evacuations take place.

21st, A blister to be applied to the breast.

27th, Seized last night with pain and constriction in region of heart; pulse 108; diastolic sound very long and grating; heart's impulse strong, especially at right side of sternum.

Stop the wine; and give twenty drops of antimonial wine every second hour.

May 2d, To have three ounces of gin.

7th, Much abdominal pain and anasarca.

Warm fomentations to be applied to the abdomen; and a turpentine enema to be administered.

27th, Orthopnœa; much dulness on percussion of left side.

29th, An ill maturated small abscess, with much swelling and redness, has appeared along umbilicus; also erythematous redness and pain of left leg.

June 3d, Death.

5th, Inspection.—The general aspect of body is bloated and highly œdematous. *Head,*—Great vascularity of the membranes of the brain, and congestion of the sinuses; brain softer than natural. *Thorax,*—Both divisions contained much fluid; both lungs adhered posteriorly, and in both the bronchial membrane was thickened, vascular, and in some places rough. The *Pericardium* contained nine ounces of dark-coloured turbid serum. Heart when washed weighed thirteen ounces. Circumference at base, 13½ inches; length of right ventricle, externally, 5½ inches; long axis of right auricle, 5 inches, and its walls diaphanous.

The right side of heart constituted a single chamber nearly, from great widening of communicating foramen. The left ventricle converted into a flaccid bag, which might hold eight ounces of fluid. Aortic orifice admitted no more than a moderate-sized middle finger; mitral opening permitted the passage of four fingers. No disease in the valves except some attenuation.

Abdomen enlarged greatly from meteorism and effusion. Liver enlarged, congested, very friable, and unctuous. Capsule easily turned off from its surface. Kidneys also in an advanced stage of granular disease; weight of each, five ounces. Weight of spleen, six ounces.

In the foregoing case the woman appears, from the tendency to be affected by cold, the sonorousness of the thorax, and the dilatation of the jugular veins, to have had a measure of habitual congestion of the right heart at the period of her labour, and the constitutional disturbance occasioned by the milk fever on the third day was probably the cause of the acute pulmonary congestion, marked by cough, tightness in the bronchi, and dyspnœa which then occurred. The symptoms had an asthenic complexion from the time of admission (requiring ammonia, and not bearing calomel); and in proportion as the regurgitation on the right heart increased, the liver got enlarged, the abdomen tumid, and the limbs anasarcous. There was a temporary abatement of the urgent symptoms, from the use of remedies fitted to diminish the portal plethora, and to support the power of the heart; but these were instantly succeeded by aortic diastolic sound, return of dyspnœa, and pulsation in the jugulars, pointing out the successive reflux on the left ventricle, the lungs, and superior cava, which was permanently subsisting. Another temporary amelioration from similar treatment was followed by a return of all the symptoms, and death.

I.—*Simple Congestive or Dextral Disease of the Heart.*

C, In its concluding stage.

Having, in the previous observations on this form of chronic heart affection, treated of it in A, its initiatory or early stage, at which time the malady is limited to a condition of exclusively pulmonary congestion, with distension, or, at most, slight dilatation of the right heart; and, again, in B, its transition period, when the lentor of the capillary ramifications of the pulmonary vessels, and the dilatation of the right heart, having become persistent, the pressure of the latter on the aorta has led to the setting up of a like stretching and dilating process in the left heart;—it remains, in the present part, to describe the influence which the pressure on the aorta, here referred to, has, in at length inducing a highly-overloaded state of the general as well as of the pulmonary circulation.

The distinctive characters of this final stage of simple congestive disease of the heart have already been referred to more than once incidentally. It is constituted by an overloaded state both of the pulmonic vessels and systemic veins, by dilatation of all the cavities of the heart, and of all its openings, excepting the aortic, united to a greater or less amount of congestion, or of disorganization of most, if not of all the other viscera of the three great cavities. The right side of the heart is no longer only temporarily distended, but it is habitually dilated; the same is true also of the left; and the liver likewise is at first permanently engorged and enlarged. The degree of congestive growth in the latter organ often exceeds a third or a half, and even sometimes is equal nearly to as much again as its natural bulk. The seats of this engorgement are the lobuli, the hypertrophy of which is obvious to the naked eye. They are most developed at the superior part of the right lobe, and soon exchange the dusky-red colour, which they have at first, for one that is paler, till at length the organ assumes a yellowish colour throughout, and becomes friable, has a greasy aspect and soapy feel, and its peritoneal covering is thickened, and often easily separable. At a more advanced period, the organ not only ceases to acquire bulk, but begins to shrink; its surface becomes indurated, uneven, fissured by indentations, corrugated, and occupied by thickened capsule or opaque bands; and its substance is either simply reduced from four or five pounds weight to two or less, and has become of very firm consistence; or it has acquired a mottled, variegated, or nutmeg aspect. In proportion as the liver becomes impervious to blood, from the changes in its structure now mentioned, it ceases to supply the overloaded heart with a convenient receptacle for its reflux blood; and should the causes of the cardiac congestion continue to operate, the lungs, the brain, the spleen, the mesenteric veins, or the kidneys, become consecutively or simultaneously engorged.

It is in such circumstances that hæmoptysis and pleuritis, or actual pneumonia, become complications of the disease; and that the lungs gradually acquire extended and often numerously-laminated adhesions to the costal pleuræ, by which their internal circulation is still farther encumbered, and their size often sensibly diminished. The spleen, also, at this conjuncture, occasionally swells out to an enormous bulk; and, in other instances, the mesenteric, the hæmorrhoidal, or the uterine veins, assume a species of safety-valve function, and, by the discharge of their contents, contribute temporarily to the relief of the cardial plethora. It is thus also usual to find the kidneys in a state of simple engorgement and hypertrophy, with the liver in the same subject in the stage of atrophy. At this time the kidneys often weigh, particularly the left, six, seven, or more ounces. They are of a uniform red colour; their molecular structure is hypertrophied; and their secretion is commonly at first increased, and contains albumen, but gradually, should the patient live, both glands pass successively through the same changes as those sustained by the liver. Their texture is found greasy, partially anæmic; their granular structure obliterated; their capsules thickened, and less adherent; and, at length, they are reduced to a small size, are indurated, and changed into nearly a homogeneous yellowish-white structure. Coetaneous with these alterations, there are, of course, the general effusion of the serum of the blood into the cavities, and into the areolar tissue, the inability of diuretics to act, and a natural close to the morbid changes by death.

I subjoin the narratives of some additional examples of the affection when thus fully consummated.

Case 17.—*Cardiac symptoms for nine years; an attack of general dropsy two and a half years before the present; abdominal swelling four months before admission, at which latter time had heart symptoms and dropsy; great relief by treatment; re-admission; and death in five days.*

John Smith, a labourer, aged 43, admitted October 21, 1847. This man has been subject, for the last nine years, to palpitation of the heart, and two and a half years ago, in consequence of exposure to cold and wet, he had general dropsy. After some treatment he is said to have had tolerable health, till four months since, when he became affected with swelling in the belly and limbs. The disturbance of the heart has become worse in the last two weeks, and he labours under great oppression at the breast, severe dyspnœa, some cough and expectoration. Obtuse angular decubitus on right side; cardiac dulness goes completely across sternum; heart's action irregular and intermittent; and heart's sounds obscured by a continuous murmur; sonorous breathing. The abdomen is large and tender over epigastric, right hypochondriac, and renal regions. Bowels regular; tongue moist and clean; pulse 100, small, irregular and intermittent; urine acid, slightly albuminous, of a deep brown colour, and suspending a light flocculent mucous cloud.

22d, Pulse, at heart, above 100, but so remarkably irregular, short, and rapid in its beats, that it cannot be enumerated; at wrist it is about 48, but can scarcely be counted; cardiac murmur is a plane sound, but is not easily heard owing to the emphysema, except when the heart lies on the diaphragm behind the xiphoid cartilage; dulness on percussion greater in sitting posture, but when he lies it is limited to about two square inches at the lower part of the sternum; œdema is confined chiefly to the face and neck, especially beneath the eyes; tongue cold and violet-coloured.

To have every fourth hour four grains of calomel, until the bowels are moved; to take every fourth hour the diuretic powder without digitalis.

Eight ounces of blood to be drawn, by cupping, from the epigastric and right hypochondriac regions.

The sinapised foot-bath for half an hour.

25th, Much catharsis after last report, with some relief to the dyspnœa; sinistral decubitus is still followed immediately by excessive cough, imperfect vision, and suspension of the breath; cough is chiefly laryngeal; liver projects a good deal at right side of umbilicus, and is indurated; some pulsatory swelling at top of sternum, and the jugular veins are greatly enlarged; an indistinct plane sound is still heard over whole of heart and in the neck.

To take every second hour the third part of one pill of calomel and opium, and continue the diuretic powder.

Nov. 1st, Urine from six to seven pounds.

To have six ounces of the solution of supernitrate of potass, and to take half an ounce every fourth hour; to continue diuretic powder.

18th, Soon after the kidneys had begun to act, all the symptoms subsided; the patient could recline with ease on left side, and walk with agility and comfort; dismissed relieved.

27th, Re-admitted; four days after dismissal, and after renewed exposure to cold, most of his former symptoms returned. Anasarca is at present considerable; the belly is large, tympanitic, and fluctuant; much dyspnœa; dextral decubitus; heart's sounds are so tumultuous, rapid, and feeble, that they cannot be counted; but a plane or continuous rasping sound is heard over whole of heart; pulse at wrist 80, irregular and intermitting. Bowels slow; tongue moist, pale, and large.

To take the diuretic powder without digitalis every eighth hour.

Dec. 2d, Patient, without obvious cause, and while in bed, was seized this morning with a sudden sense of shock in the head, constriction of thorax, vertigo, and loss of vision, and died in the afternoon.

Inspection, thirty-six hours after death.—The upper aspect of thorax and the face were very œdematous, and stained by ecchymoses; the thorax and abdomen were filled with serum, that in the former bloody. The pericardium contained about two ounces of fluid, and the heart was observed to stretch more to right side than usual; its long diameter was eight inches, and its weight, inclusive of pericardium and vessels, two pounds ten ounces; the surface was pale. The walls of the right ventricle were about one fourth of an inch, and those of left about a whole inch in thickness; the cavity of the former could have held twelve, and that of the latter eight fluid ounces; the right auriculo-ventricular opening permitted the passage of all the fingers; that of the pulmonary artery of two laid horizontally. The left auriculo-ventricular opening admitted three and the aortic orifice one full-sized finger only. Some soft warts were observed on the valves of the aorta. These and other valves were without thickening.

Both lungs were highly emphysematous; a few adhesions were seen on the left, which weighed twenty-two ounces; while the right lung, in consequence of serous infiltration, weighed thirty ounces.

The liver was pale, smaller than common, of very firm contexture, and weighed three pounds ten ounces. The spleen was soft and diffluent, and five ounces and a half in weight. The right kidney weighed seven ounces and a quarter, and left seven ounces, soft, vascular, and slightly marbled with white tissue.

Case 18.—*Chronic catarrh for four years; aggravation of symptoms for four months; recovery in five weeks. Readmission six weeks afterwards, with anasarca; great dyspnœa and hæmorrhoids; death in less than one month. Inspection.*

John Kelly, aged 30, a weaver, admitted November 5, 1846. This man had, in consequence of exposure to cold four years ago, an attack of catarrh, which left permanent cough. A twelve-month afterwards the cough became very severe, and was accompanied by dyspnœa and hæmoptysis. He then came into hospital, and after being under treatment for nearly three months, was dismissed, a little cough only remaining. He continued in pretty good health until four months ago, when cough and dyspnœa became more troublesome, and have gradually been becoming worse since. On admission he is debilitated; complains of want of sleep; bowels slow; has some appetite, and tongue is pretty clean; pulse 74; breathing tranquil while he is at rest, but dyspnœa easily excited by change of posture or motion. The thoracic parietes are exceedingly distended and prominent; breathing abdominal; precordial dulness absent. Heart's sounds exceedingly faint; pulsations visible in epigastrium; scarcely perceptible to the hand over ribs. Sonorous, sibilant, or dry subcrepitant rattles are heard over almost every part of the chest, in some places accompanying inspiration alone, in others, both inspiration and expiration. They are loudest superiorly and in front; comparatively faint and distant over posterior surface of chest. Sputum frothy and slightly opaque. Lips of a rather dark colour, and surface inclined to become cold. No œdema.

Two ounces of sulphate of magnesia to be dissolved in one pint and a half of water, and the patient to take of this one large glassful every third hour until the bowels are moved. To have the feet immersed in hot water every evening.

15th, A blister to be applied over the breast bone for four hours. Twelve pills consisting each of one squill pill combined with one grain of ipecacuan; the patient to take one three times daily.

21st, Dyspnœa often urgent.

To smoke extract of stramonium. To rub over chest tartar-emetic ointment. To add to each of the pills half a grain of calomel and a quarter of a grain of opium, and continue as formerly.

Dec. 14*th*, Dismissed relieved.

Jan. 26*th*, 1847, Readmitted—complaining of cough, dyspnœa, great anasarca, some ascites and meteorism of bowels; face purple and swollen; pulse 116; sputum semipurulent and tenacious; respiratory sounds very faint; cardiac sounds heard extensively even behind; bowels loose; hemorrhoids bleed.

To have three ounces of gin daily.. To take the diuretic powder without digitalis, every sixth hour. To use imperial for drink. To have three drachms of castor oil every second night.

Feb. 2*d*, Grating diastolic sound heard only about two inches nearer sternum than left mammilla; much coldness of surface; pulse scarcely perceptible at wrist; heart's sounds feeble.

Gin to be increased to six ounces; vessels filled with hot water to be placed around person.

Increased anasarca and oppression, with tendency to sleep.

To each dose of the diuretic powder one grain of squill to be added; and one to be taken every third hour.

Six grains of calomel and as much electuary of catechu as will form it into twelve pills; of which one to be taken every fourth hour; other remedies to be continued.

13th, Gin has been lessened to four ounces daily for last two days. There is greatly-increased lividity of surface, with feeble heart sounds, and great preponderance of second. Much swelling of inferior extremities.

Gin to be increased to eight ounces.

19th, Sphacelus of right thigh with serous exudation, and death on 20th.

Inspection.—The lungs adherent universally by old adhesions; they were infiltrated with serum, and highly emphysematous. The bronchial lining was stained throughout of a deep red colour; some blood, but no pus, was effused within the tubes. The bronchial glands were enlarged. The heart weighed eighteen ounces. The left ventricle and other cavities were enlarged. The aortic orifice did not transmit two fingers. The auricular openings each transmitted three. The valves were healthy.

The liver was small, weighing only twenty ounces. The proper capsule was thick, firm, and opaque; its substance pale and very dense. The spleen weighed five ounces and a-half. Tissue quite black, but of moderate consistence.

The kidneys were granular; surface rough, and of a liver-brown colour; their texture was easily broken down. That of right side weighed six, that of left six ounces and a half.

Peritoneal sac contained upwards of four pounds of highly-coloured serum.

Case 19.—*Puerperal albuminuria probably of some standing; followed during another pregnancy by bronchitis of nine months' duration; and, after parturition, by anasarca; continuous rubbing sound over whole of heart; dyspnœa, progressive increase of symptoms, and death, in two and a-half months. Inspection.*

Mrs Pollock, aged 40, admitted 5th October 1846. This woman has been pregnant five times; first time twenty years since, and the last three weeks. On every occasion had dropsical swelling of the lower extremities, which always disappeared after delivery, till the present time, when it has continued greatly to increase. Nine months ago, in consequence of exposure to cold, she was seized with catarrhal symptoms, which continue, having become worse about six weeks since. Anasarca general; abdomen tympanitic, and also fluctuant. Retches much; has palpitation increased by exertion, and accompanied by vertigo and confused vision. The heart sounds are obscured by a continuous murmur, which is heard indifferently at edge of axilla, and at the cartilages of the third ribs on both sides. Diffluent purulent expectoration, with diaphragmatic breathing. Sibilous, intermingled with sonorons rattles, and some moist crepitous rattle over thorax. Pulse 92. Tongue purple, with white coating. Urine slightly albuminous; specific gravity 1·023, slightly acid, and of a deep orange colour.

To have the fourth part of a pill of calomel and opium every third hour. To take the diuretic powder without digitalis every eighth hour. To have two scruples of the compound jalap powder every sixth hour.

11*th*, To have half a drachm of compound jalap powder every third hour till the bowels are moved. To take the diuretic powder every fourth hour.

17*th*, Immediately after last report the gums became slightly tumid, and the diuretics began to act, the urine averaging about eight pounds and a-half daily, with much relief of the breathing. For last three days the dyspnœa has become permanently so much aggravated as to compel her to remain erect. The thorax is resonant throughout, except in the cardiac region, the dulness in which stretches from mesial line of sternum, along the level of the third and seventh ribs, to outer aspect of mamma. Sputum opaque and nummular; respiration diaphragmatic; starts frequently from sleep, with sense of suffocation. The cardiac impulse gone; heart sounds ringing over ventricle, with great harshness of the diastolic over aortic orifice. There is no murmur accompanying the sounds, except in the neck, where there is a continuous souffle. Cannot recline on right side. Pulse 108.

To omit calomel and opium pills, and the diuretic powder. A blister to be applied over the sternum. To have daily four ounces of port wine. Half a drachm of iodide of potassium to be dissolved in four drachms of tincture of cantharides; and ten minims of the solution to be taken in water three times daily.

23*d*, Cardiac dulness continues, except under mamma, where there are resonance and tumultuous pulsation.

Repeat the blister over sternum. The solution of tincture of cantharides and iodide to be increased to fifteen minims three times daily.

Nov. 3*d*, Urine, which, after last report, remained at about ten pounds daily, has become more scanty.

The diuretic powder every four hours, and continue other remedies.

8*th*, Respiration natural, and she can recline horizontally on either side. General feelings improved; but the legs and thighs continue to be much swollen; and there is a distinct murmur over both sounds of the heart, which are coalesced into a continuous single sound.

To have half an ounce of bitartrate of potass morning and evening. Other medicines to be continued.

23d, Increase of anasarca and chest symptoms for some days. Double cardiac murmur heard over mitral and aortic regions. The liver projects in the epigastrium, and is hardened. General dropsy great. Urine about eight pounds.

On the 8th, she is reported as suffering under serious increase of all the symptoms, with œdematous engorgement of the left lung (the side on which she reclined when she could do so), and expectoration of intensely white and glutinous sputa, mixed with innumerable minute bullæ. The calomel and opium, solution of carbonate of ammonia, and a daily allowance of gin, were resorted to; and her existence was protracted till the 19th December, when she died.

Inspection.—The thorax contained four gallons of light amber-coloured serum. The right lung was twenty-two ounces in weight; the left sixteen ounces. An abscess, of the size of a small billiard ball, was situated in posterior part of the latter, and communicated with the bronchi, which were very generally congested and thickened in their inner lining.

The pericardium occupied a large part of chest; its diameter across was 7½ inches, and it contained about four ounces of serum, The size of the heart itself was equal to about that of the subject's two clenched fists. All the cavities were dilated, and the walls slightly thickened, but the valves were healthy. Both auriculo-ventricular openings admitted the easy transit of three full-sized fingers, the pulmonary artery of the index and middle fingers, and the aorta of the thumb. The weight of the organ was twenty ounces.

The abdomen contained about eight gallons of fluid. The liver was of large size, and weighed seventy ounces; the capsule was opaque at various points, and substance of gland soft and granular. Spleen highly engorged, and weighed nine ounces.

The united weight of both kidneys only eight ounces. They were small, hard, and blanched in their whole structure.

In reviewing the foregoing cases, in connection with the other details of this inquiry, the extent of pathological change, to which a varicose condition of the capillary pulmonic vessels gives origin in every organ nearly of the body, appears most remarkable, and is also fraught with practical importance. In reference to acute cases, the justice of such a statement will fully appear, when the numerous and severe complications, with which the attacks of the advanced disease are uniformly accompanied, are considered, the limits of these being often in reality those only of the organization. But, besides these, a respectable proportion of our consultations in the summer months is formed of cases in which the symptoms submitted to medical observation and treatment have, either immediately or through the intervention of a second organ, the origin in question, although, because the urgency, and perhaps the presence, of the bronchial irritation have passed away, this is probably not even suspected. I need only refer here to the examples of headach, *tinnitus aurium*, deafness, and other signs of serious injury in the cerebro-spinal axis with which we are thus conversant, or to the multiplied forms of circulatory and splanchnic disease, which, from the same source, come under our notice.

Another point of much interest in the history of dextral disease of the heart is, that every secondary alteration or lesion which occurs in its course involves within it a remedial principle or sanative effort, however imperfect. The structural changes which are set up, become practically serious additions to the severity of the case, although, abstractly, they each appear to grow spontaneously out of a curative action, for the alleviation of some preceding morbid condition. Thus the progressive dilatation in succession of the pulmonary artery, right heart, great systemic veins, and portal system, which is the essential feature of the fully-formed disease, is at once an immediate effect of the pulmonary lentor, and also an obvious mode of relieving this. The overloaded right heart ultimately obstructs the circulation of the left; but it primarily relieves the vascular engorgement of the respiratory system. The distension in the sinuses of the brain, of the liver, the spleen, the intestines, and kidneys, which at length occasions most disastrous changes in the capillary extremities of these respective vessels, affords for long most invaluable relief to the overstretched and oppressed right side of the heart. And it seems to proceed from the operation of a similar law, and in the first instance with the effect of bringing also a certain amount of corresponding relief, that the blood itself becomes successively disburdened, sometimes of a portion of its whole contents, as by an attack of hæmoptysis, a cerebral, a jecoral, a uterine, or other hemorrhage; or, more frequently, of one or more of its constituent parts, as its fat, its albumen, or its serum. The profuse sweatings, accompanied, when the arterialization of the blood in the lungs is greatly hindered, by excessive coldness of the surface; and the extreme diuresis, sometimes passing into actual diabetes; and, lastly, the catharsis;—all of which occur in certain instances of simple cardiac congestion, appear to belong to the same category, and furnish important suggestions in regard to the proper indications of treatment in this affection.

A specialty in all of the complications of dextral cardiac disease, to which attention has been here directed, is, that they are thus sequences as well as complications. In this respect they differ from complications in a more ordinary sense, in that they constitute extensions of the primitive malady rather than distinct and separate diseases. They are new members, but still integrant parts, of the original morbid condition, which had its point of departure in the minute blood-vessels of the air-cells of the lungs. Thus, in the man Smith (case 17), the albuminuria, which was plainly, —as shown by the enlarged, heavy, and vascular state of the kidneys,—in its first stage only at the time that the parent disease was in its last, the new element of granular kidneys was at once a complication and a sequence or effect; while, in the woman Pollock (case 19), the dextral disease of the heart was accidentally engrafted on an individual labouring previously under albuminuria from other causes. In this patient the structural lesions of the liver and kidneys were respectively the very reverse of what is the ordinary arrangement, or of what obtained in Smith. The liver was enlarged to seventy ounces, and the kidneys were shrunk to an average of four ounces each, their tissues being indurated as well as atrophied; that is, the former viscus was in the first stage only of granular and adipose degeneration, instead of being, as is usual at the termination of simple congestive cardiac disease, in the last, while the kidneys presented the most advanced form of this disorganisation, rather than any of its earlier varieties.

With these remarks premised, I shall, in the succeeding part of this memoir, offer a few illustrations of the different terminations or complications of dextral disease of the heart, with some observations on its most appropriate treatment.

(To be continued.)

Art. II.—*Medical Remarks on Emigrant Ships to North America.* By Thomas Stratton, M. D. Edin.; Surgeon R. N.; Memb. Edinburgh R. I. Clinical Soc.; Memb. Toronto Medico-Chirurgical Soc.; Memb. Montreal Natural History Soc.; Corresp. Memb. Quebec Literary and Historical Soc.; Hon. Memb. Highland Soc. of Canada, &c.

In the Edinb. Med. and Surg. Journal for July 1848, volume seventieth, I gave an account of the ocean-plague which made victims of so many of the emigrants to Canada in 1847, and I suggested various improvements in the way of managing emigration. My observations on these subjects passed through seven editions; they appeared in Dr Hall's Montreal Medical Journal, in three Montreal newspapers, in Simmonds' Colonial Magazine (London), and in pamphlet form. Of this last edition, copies were sent to Earl Grey, colonial secretary; Mr Hawes, M.P., under colonial secretary; and many other members of parliament and influential persons. Some great improvements in the manner of conducting emigration that were made after this, perhaps arose partly from my having exposed the faults of the former plan. In the Edin. Med. and Surg. Journal for January 1849, volume seventy-first, I gave some additional statistics of the emigrant mortality of 1847. I have now crossed the Atlantic ten times, and the time before last I officiated as surgeon of a passenger-ship.

In the present communication:—

1. Some particulars relating to emigrant ships in 1847 will be given, which will probably interest the medical reader, and the philanthropist.
2. There will be shown the necessity that there is for there being a surgeon in every emigrant ship.
3. Without any reference to sanitary reasons, it will appear that it would now be to the pecuniary advantage of the ship-owner, that his ship carried a surgeon.

In May 1849, when sailing up the St Lawrence in the *Laurel*, we had on board for some days a very intelligent pilot, a French-Canadian named Girrord. He informed us, that in the sickly season of 1847, he remained at home the first part of the summer, and would not go as pilot. This was done also by many other pilots; and those who did go out used to avoid emigrant ships as much as possible. Generally they could tell them at a distance from seeing clothes drying in the rigging, from their sides being dirty, and seeing the crowd on deck. The second mark was the one most to be depended on, as, when captains found the reluctance of pilots to come on board, and be exposed to the infection of typhus, they prevented the passengers from appearing on deck; and when hailed by a pilot and asked if the ship carried emigrants, an answer was given in the negative. Towards the close of the season, Girrord went down the St Lawrence as pilot of a ship, and when about to return, having no boat of his own, and seeing a ship which he judged not to be an emigrant vessel, he hailed and inquired if they had passengers. They replied no; and as there were no clothes drying in the rigging, or crowd visible on deck, and as the captain had taken the precaution of having the ship's sides cleaned, he believed them. They sent their boat for Girrord, who as soon as he got on deck observed the odour of emigrants. The boat was hurriedly hoisted up, and now the deception was manifest, and crowds of passengers came up on deck. The ship was the *Erin's Queen* from Liverpool; many persons were lying sick, about forty passengers had died, and likewise ten of the crew. Girrord had his last pilotage-fee with him, about L.30, and he offered this to the captain if he would land him, but he refused. The generality of the passengers had only an insufficient quantity of sour flour to subsist on, and this they took mixed with cold water. When a death occurred, there frequently was difficulty in inducing the passengers to carry the body on deck; and some corpses were three, five, or more days lying in the berth. They never sewed them up in canvas or attached any weight to sink them, but simply threw them over the side. From fear of infection, the pilot would not go below into the cabin, but he and the captain lived in a tent on deck. The wind being contrary, they had to tack up the river, and every tack they threw overboard four or five bodies. They now began to throw them overboard the moment they were dead. Off Basque island, they threw twenty corpses overboard. A little further up the river, the captain left the ship on the pretence of proceeding to procure a steamer to tow the vessel; but the pilot at the time considered that his leaving was a simple act of desertion from his fear of the fever, and it was proved to have been so by his not returning either with or without a steamer. The captain (whose name I suppress) died a short time after this, but not of fever. After being three days on board, another pilot came near them, and having induced him to take charge of the ship, Girrord left the ill-fated vessel. This second pilot was seized with fever and died. A breeze now sprung up, and the ship reached the Grosse Isle quarantine station (thirty-two miles below Quebec). Here in the evening they sent eleven bodies ashore in a boat to be buried, when the authorities told them it was too late that day for them to be interred, and directed them to bring them back next morning. They rowed the boat back to the vessel, leaving its melancholy freight in it; in the morning they found that the boat, being very leaky, had quite filled with water, and the corpses having floated off, were nowhere to be seen. But eleven or twelve more passengers had died on board during the night, and their bodies taken on shore represented to the quarantine authorities the other eleven of the night before. All the above particulars I had from Girrord, whom I considered quite worthy of credit. The *Erin's Queen* had 493 passengers, of whom one hundred and thirty-six died. The scenes of misery on board of this vessel could hardly have been surpassed in a crowded and sickly slaver on the African coast.

As another instance of the condition of these ships, I may mention that Dr Liddell informed me, that on board the *Fame*, which arrived with a number of emigrants in 1847, the passengers during the whole voyage, when attending to the calls of nature, had invariably done so into the hold, some of the lower deck planks being lifted for the occasion. The vessel was in ballast; fortunately she had a short passage; if she had had a long one, there would most likely have been much disease on board; as it happened, there were no deaths.

In the spring of 1849, having occasion to proceed on private affairs from Britain to Canada, with the intention of returning after a few weeks' stay, I went on board of several ships at London, about to sail for the St Lawrence, and chose the largest one, being the *Laurel.* She belonged to Quebec, and was one of 808 register-tons, although advertised by the London brokers as being of 1000 tons. We sailed on the 7th of April, and arrived at Quebec on the 30th of May, after a long passage of fifty-three days, the average passage being forty days. The day of sailing was put off for three or four days, and I did not engage to go as surgeon, till three days before she sailed. It was only on the last day, that she was advertised to carry a surgeon, and before this time almost all the passengers had taken their passage. What is done by the brokers in these cases is, when a medical man speaks to them of going out in a ship,they endeavour to discover if he is going out to settle, and if so, they hesitate even about making any deduction from his passage-money. I told them I was going out, not with the intention of settling, but with the intention of returning in a few weeks, and so, all that was left to the brokers to suppose was that I was going out merely for remunerative reasons. They played off against me, as is their custom, two surgeons, one of whom I am pretty certain was a man of straw. He was said to be one who was going out to settle in Canada, and offered to act as surgeon on condition of receiving a free passage. The other was a gentleman of about sixty-five who wished to go out to settle. There was some doubt, nevertheless, of his being a medical man at all, and I heard of his having shown to the government emigration agent an affidavit taken by himself before the Lord Mayor, that he had been "in practice" before the year 1815, when the Apothecaries' Act came into operation in South Britain. In this country,—which, from the usual questions about one's health, a sarcastic foreigner has styled a nation of physicians,—it would be difficult to find a person who had not given medical advice to some one; and such a lax way as the above, of any person of a certain age giving himself a medical or surgical diploma, is open to serious objections. The brokers said that the second gentleman mentioned above had offered to go out for a free passage only, and without any remuneration in addition. The captain, however, said that he would rather pay me *so much* than take the other for nothing; from which I inferred that the other candidate had asked a sum larger than the one I had agreed to take, and which was less than at first demanded. After an oral engagement, they made an unsuccessful attempt to add, in a written agreement, the performance of some non-medical duties. I mention these details merely for the purpose of warning any medical man going on a voyage either as a passenger, or specially to act as surgeon, against the meanness, tricks, cheating, and imposition generally practised by saving captains, avaricious owners, and rapacious brokers. In the port of London, when one engages as surgeon of an emigrant ship, this is conditional on his afterwards satisfying the government emigration agent (a Lieut. of the R.N.) that he is in reality a medical man; as far as the brokers know, or care to know, any non-medical person might engage as surgeon of a ship going on a long voyage. At Grosse Isle, in May 1849, Dr George Douglas, the quarantine-superintendent, told me that some of the persons who professed to be surgeons of emigrant ships arriving there, he was quite certain (from conversing with them) had not the slightest claim to the designation of medical men. If ship-owners offered sufficient remuneration, they would easily get qualified medical men to accept the appointment.

Medicines for Merchant Ships.—Before mentioning the quantities of medicines ordered for this emigrant ship, I may make a few remarks on the government scale of medicines for merchant ships. In pursuance of the Merchant Seamen's Act, 7th and 8th Victoria, chap. 112, clause 18, the Admiralty, on the 16th December 1844, made it compulsory for merchant ships to provide themselves with the following scale of medicines:—

	lb.	oz.	dr.		lb.	oz.	dr.
1. Castor oil	1	0	0	16. Spirit of hartshorn	0	4	0
2. Epsom salts	4	0	0	17. Paregoric elixir	0	4	0
3. Powdered jalap	0	4	0	18. Essence of peppermint	0	1	0
4. ——— rhubarb	0	4	0	19. Alum	0	8	0
5. ——— ginger	0	2	0	20. Blue stone	0	4	0
6. Carbonate of soda	0	8	0	21. Quinine	0	0	4
7. Tartaric acid	0	8	0	22. Calomel	0	4	0
8. Senna leaves	0	6	0	23. Dover's powder	0	2	0
9. Sulphur	0	8	0	24. Mustard	1	0	0
10. Cream of tartar	0	8	0	25. Mercurial ointment	0	8	0
11. Nitre	1	0	0	26. Lint	0	4	0
12. Laudanum	0	4	0	27. Simple cerate	0	8	0
13. Tincture of rhubarb	0	8	0	28. Basilicon ointment	0	8	0
14. Balsam of copaiba	0	8	0				
15. Opodeldoc	0	8	0				

Pills.

29. Four dozen purgative (Pil. Coloc. co., Edin. Ph.)
30. Four dozen blue pills.
31. Two dozen opium pills (Edin. Pharm.)

Powders.

32. Two dozen purgative, each containing a drachm of compound jalap powder, and two grains of calomel.
33. Two dozen emetic, each containing fifteen grains of ipecacuanha, and two grains of tartar emetic.

34. Two dozen sudorific, each containing ten grains of nitre, ten grains of cream of tartar, and five grains of Dover's powder.
35. Two dozen injection, each containing three grains of the acetate of zinc.

The chest also contains scales and weights; mortar and pestle; a tile; measure-glass; funnel; pewter-cup; two pewter teaspoons; spatula; a pair of scissors; a syringe; two lancets; six bandages of different sizes; needles, pins, and thread; six yards of calico; a bottle of Bullin's blistering fluid; and two yards of adhesive plaster.

The above are the quantities of medicines for ten men; and the quantities are to be increased proportionally, for any additional number of men there may be in the crew.

Remarks on the Scale.—I have heard that, at the request of Sir William Burnett, the above scale was drawn up by the late Dr Arthur, Surgeon R. N. The length of time the quantities are intended to last is not mentioned, but is supposed to be one year. This, however, would better not be left to doubt, as some voyages, such as South Sea whaling voyages, last three years. It would be a good addition to the scale, to insert two ounces of acetate of lead, four ounces of the solution of acetate of lead, four ounces of chloride of lime, two ounces of antimonial wine, and two pints of Sir W. Burnett's solution of chloride of zinc. It may be convenient for some of my readers to know that the above scale of medicines may be found mentioned at length in the *Mercantile Navy List,* and in Mr Palmer's *Guide-book to the Government Medicine-chest* (18mo, London, 1849).

Medicines for Emigrant Ships.—There is no authorised or compulsory scale for emigrant ships. They carry a chest for the crew in accordance with the Act, and a separate chest for the passengers; usually, if there is no surgeon, the broker tells a druggist to make up a chest, and not to charge more than (perhaps) L.5 for it. The druggist is apt to fill it with a few cheap and bulky articles. The following are the medicines and quantities I ordered for the *Laurel,* reckoning on two hundred passengers, and on a voyage of the possible duration of ten weeks. Of four or five articles I shall mention a larger quantity than was ordered.

Medicines and quantities for two hundred emigrants for ten weeks.

	lb.	oz.	dr.
Acid. sulph. dil...............	0	2	0
Acetatis plumbi...............	0	8	0
Acid. acetic...................	0	8	0
Acid. tart......................	0	8	0
Acid. citric.....................	1	0	0
Antimon. tartrat.............	0	2	0
Balsam copaiba...............	0	4	0
Chloride of lime.............	0	4	0
Calomel.........................	0	1	0
Cret. praep.....................	0	6	0
Solution of chloride of zinc (Sir W. Burnett's disinfecting fluid.....)	24	0	0
Creosote........................	0	0	2

	lb.	oz.	dr.		lb.	oz.	dr.
Chloroform	0	1	0	Sulphur	0	8	0
Carb. sodæ	0	10	0	Secale cornut	0	0	4
Carb. magnesi	0	8	0	Spirit. terebinth	0	4	0
Emplast. lyttæ	3	0	0	Spirit. ammon	0	2	0
Gregory's powder	0	8	0	Sp. æther. nit	0	2	0
Hydrargyr. cum creta	0	2	0	Spirit. æther. sulph	0	2	0
Liq. ammon. fort.	0	2	0	Spirit. æther. aromat	0	2	0
Liq. acetatis plumbi	2	0	0	Sulphate of bebeerine	0	1	0
Nit potass.	0	6	0	Sulph. quinæ	0	0	4
Nit. argent.	0	0	1	Sulph. magnes.	6	0	0
Ol. anisi	0	0	1	Sulph. zinci	0	0	4
Ol. croton	0	0	1	Sulph. cupri	0	0	2
Ol. menth	0	0	1	Tinct. capsici	0	8	0
Ol. oliv	0	4	0	Tinct. camph. comp	0	4	0
Ol. ricini	4	0	0	Tinct. catechu	0	4	0
Pulv. ipecacuanh	1	0	0	Tinct. opii	2	0	0
Pulv. ipecacuanh. comp.	0	4	0	Ung. cetacei	5	0	0
Pulv. opii	0	1	0	Ung. hydr. mit	0	8	0
Pulv. rhei	0	3	0	Ung. hydr. nitratis	0	0	4
Pulv. zingib.	0	1	0	Vin. antim. tart	0	8	0

A. Six dozen pills, each containing, ext. coloc. co., gr. ii.; calomel, gr. i.; pulv. scammon., gr. i.
B. Six dozen pills, each containing four grains of blue pill, and half a grain of rhubarb.
C. Four dozen pills, each containing one grain of opium.
D. Four dozen pills, each containing five grains of calomel.
E. Six dozen pills, each containing, calomel, gr. ii., opii., gr. ¼.
F. Four dozen pills, each containing, ext. hyoscyam., gr. ii.; pil. hydr., gr. i.; pulv. ipecac., gr. i.

The chest also contained four dozen Seidlitz powders; ounce and minim glass; scales and weights; mortar and pestle; thread; bandages; calico; corks; eighteen one-ounce phials; eight six-ounce bottles; spatula; two syringes; two lancets; twelve leather-lined splints; and two yards of adhesive plaster.

The above medicine-chest was to cost about L.7. The taste of practitioners varies so much, that hardly any two would agree about a scale; but the above selection, both as regards substances and quantities, I can, from experience, recommend for emigrant ships to North America. I may add that, at the end of the voyage, some of the articles were untouched, and many very slightly diminished in quantity. Some may consider that some of the quantities are unnecessarily large; but, in case of dysentery being very general, it is well to be provided amply with the preparations of lead and opium; ipecacuanha in the event of measles; and tincture of capsicum for sea-sickness.

Number of Passengers, &c.—The crew numbered 29, and had accommodations on deck, in what was called *the house*, and three or four slept in the forecastle. The seven cabin passengers, and eight second-cabin passengers, occupied the poop. The *betwixt-decks*, or passengers' deck, was seven feet high, and had on each side two tiers of bed places, their ends being to the ship's side. The after part was occupied by the intermediate passengers, who had a sliding-door to their berths, but in dietary and other respects were situated similarly to the steerage passengers, and I shall speak of both under the latter name. The intermediate and steerage passengers numbered 217; of whom, 69 were males, and 62 females, above fourteen years of age; 40 were males, and 27 females, between one and fourteen years; 11 were males, and 8 were females, under one year. Of the 217 steerage passengers, 84 were under fourteen years; they, with four children in the second-cabin, and one born on board, made 89 children in all, a much greater proportion than is usually seen in emigrant ships. The 217 steerage passengers made what the ship's papers were pleased to call "175 *statute adults.*" The total number of persons on board was 261. The steerage passage, including provisions, and without bedding, was L.6, 5s. to Quebec, or L.6, 10s. to Montreal; the intermediate passengers paid L.1 more; under fourteen years, half of the above sum; and, under one year, the passage was free. The Passengers'-Act requires that a surgeon inspect them, to see that the ship does not sail with any labouring under any infectious disease; it also alludes to the fee, which I think is L.1 for every hundred persons, and which the broker takes care not to pay, if the surgeon is not aware of the regulation.

Diseases.—The following is a list of the diseases of which instances occurred on the passage, and the numbers show the number of cases of each:—ague, 5; boils, 3; burn, 1; catarrh, 4; colic, 1; constipation, 4; infantile convulsions, 1; croup, 1; delirium-tremens, 1; dyspepsia, 2; diarrhœa, 7; dysentery, 49; gonorrhœa, 2; heart-affection, 1; hysteria, 3; infantile remittent fever, 1; leucorrhœa, 1; measles, 30; miscarriage, 2; ophthalmia, 1; pneumonia, 1; prurigo, 2; parturition, 1; rheumatism, 3; sea-sickness, ; tabes mesenterica, 1; toothach, 3; threatened apoplexy, 1. I copy this list from my daily sick-book, and I have no doubt that some cases were neglected to be noted. Of *accidents,*—a seaman fell from aloft, a height of about eighty feet, to the deck; a boy fell on his head into the hold, a height of about twelve feet, and fractured the nasal bones; and a passenger slipped down a ladder, and fractured his fibula. The mere enumeration of these diseases and accidents is sufficient to convince all persons, except ship-owners and ship-brokers, of the necessity that there is for an emigrant ship to carry a surgeon. I shall now make a few remarks on some of the diseases.

Sea-sickness.—It is of course only the more severe and tedious cases of this affection which call for medical notice. The best preventive is bodily or mental occupation. Five, ten, or fifteen drops of tincture of capsicum, either alone, or with a few drops of tincture of opium, is a good remedy for checking the vomiting; and a few drops of the former before meals is useful till the stomach becomes less irritable. If the bowels are confined, a few grains of calomel is the best kind of purgative. If the vomiting be severe or long-continued, a sinapism or a blister ought to be applied to the epigastrium. When sea-sickness continues for one or two weeks, the stomach acquires such a habit of rejection that the case becomes very obstinate; and on a long voyage, as to Australia or India, it is quite possible for sea-sickness in a very delicate person to end fatally. We had a number of women nursing; generally, when one of them was sea-sick, the milk continued of nearly the same quantity and quality for about two days; then it became less abundant and less rich, as was shown by the limbs of the infant being less firm, &c.

Delirium-tremens.—The case of this disease occurred in a man, aged 55, by trade a shoemaker. It was his second attack, and came on after four days of unwonted and unavoidable abstinence from spirits. During the night, when talking deliriously, his talk was of his trade, and he said he was making a pair of shoes for the queen. He was treated with opiates at night, purgatives or laxatives in the mornings, and a blister to the nape of the neck. During convalescence he had also two bottles of port.

Ague.—The cases of ague were in agricultural emigrants, who had had the complaint when living near the Kentish marshes, and in whom it was revived by damp beds on ship-board. They were treated with sulphate of bebeerine, as described by me in the Edinburgh Medical and Surgical Journal for October 1849.

Parturition.—On Saturday, the 14th of April, Mrs Welsh was delivered of a male child. The labour commenced at noon, and lasted three hours. I was curious to observe how this patient would have been attended to, if no surgeon had been on board. Mrs W. had previously selected two women, on account of their skill, to attend upon her. If I had left them to themselves, they would have placed on the cord only one ligature, and this consisting of two or three fine threads, and also about eleven inches distant from the umbilicus! The day was very stormy, the ship rolling very much, and, as it was sooner than expected, I am inclined to think that the accession of labour was hastened by alarm at the rough state of the weather. The mother and child were landed in good health.

Miscarriage.—I allude to the obstetric cases, chiefly for the purpose of considering whether a stormy state of the weather had any influence on them. May 4. Mrs L. never had any children; had a miscarriage four months ago; is three months gone in pregnancy, and, at 4 P.M. to-day, had a miscarriage. She did not apply till the day after. The weather was not stormy.

May 17. Mrs A. has had seven children; had a miscarriage four months ago; to-day, at 1 A.M., had a miscarriage, the fœtus appearing to be about three and a-half or four months old. She did not apply till it was over. The weather was not stormy: if it had been so, one would have been apt to attribute these two miscarriages to alarm at the supposed or real danger.

Measles.—Of the eighty-nine children, twenty-nine were attacked with measles (also one adult), leaving sixty children who either escaped an attack, or had had it before. I intended to ascertain accurately how many of the children had passed through measles before embarking; but this I did not do completely. I am inclined, however, to consider that about thirty had done so, and this would leave thirty who were unprotected by a previous attack, and who escaped an attack on board. Of the twenty-nine children, fifteen were females, and fourteen males; the adult was a male. The treatment was generally powdered ipecacuan, given in water, with a common cataplasm applied very hot and frequently to the chest; in some cases a sinapism, and in others a blister. It was totally impracticable to separate those attacked from the others.

Prurigo.—A speedy and apparently permanent cure followed the use of a lotion, composed of two drachms of Sir William Burnett's solution of the chloride of zinc, and a pint of water, being one part to sixty parts of water. The eruption disappeared after a day or two, and did not return during the remainder of the voyage.

Hysteria.—There were several instances of "hysterics." I think it likely that in a severe and lengthened hysterical fit the employment of chloroform would be of service.

Constipation.—At the commencement of a voyage, persons in the condition of steerage passengers are apt to suffer from constipation, probably from having fewer vegetables in their diet, and from less facility of access to a water-closet, than when on shore. On Saturday, 21st April, Harriet P., aged 17, applied for laxative medicine, and stated that her bowels had not been opened since leaving London, a fortnight before. I had no reason to disbelieve her; she stated that she had not suffered any inconvenience from it. She had castor-oil, and did not apply again till Tuesday, when she had had only one motion. There were several other similar cases.

Diarrhœa.—There were seven cases of diarrhœa, all steerage passengers; four children, and three adults. None of them form part of the forty-nine dysenteric cases. The water of the St Lawrence is potable about forty miles below Quebec; but if not absolutely necessary, it is better to advise new arrivals not to drink it, as generally diarrhœa for a few days follows its use. If a new arrival at Quebec, after recovering from diarrhœa arising in this way, go on to Montreal, and remain there some weeks, he will likely have a second attack of diarrhœa from the water there; and, after some weeks there, if he travel another 180 miles to Kingston, there will probably be a repetition of the same. In 1838, when proceeding from Quebec to Kingston, in medical charge of about 150 seamen and marines, we travelled through the Rideau canal, and, on the way, about a third of the party had a slight painless diarrhœa; a pill containing a grain of opium sufficed to check it in all the cases.

Dysentery.—There were 49 cases of dysentery; 46 cases among the 217 steerage passengers, and three cases (one female adult, and two children) among the eight second cabin passengers. None of the cabin passengers or crew were attacked. The family, of which there were eight, in the second cabin, had other three members among the steerage passengers, and these three passed half of their time among either class of passengers. Of the 46 steerage cases, 27 were children (among 87 children), and 19 were adults (among 133 adults). The reason why there was a greater proportion of children attacked, perhaps, was because they were less on deck, and more in the close and dysenteric atmosphere of the passengers' deck than the adults.

Treatment of Dysentery.—In the children,—at first, castor oil and laudanum, and then several doses of mercurial chalk and Dover's powder; poultices, or a sinapism, or a blister to the abdomen; mint-water; chalk-mixture; acetate of lead, and opium.

In the adults,—a dose of castor oil, and a few drops of tincture of opium; then four or five doses of calomel and opium; a sinapism or blister to the abdomen; some had two or three blisters applied. Nearly all the children and all the adults, except two or three, had acetate of lead and opium. Neither venesection, nor leeches, nor affecting the mouth by calomel, were had recourse to. In threatened dysentery in young infants (not included in the 49 cases), the frequent application of very hot common cataplasms to the abdomen was of great service.

Is Dysentery infectious?—There are four opinions on this subject;—that it is not infectious,—that it is infectious,—that it is so only occasionally,—and, that it is so only when complicated with infectious fever; (see Dr Craigie's Practice of Physic, i. 920). And each of these opinions has eminent members of the profession among its adherents. I shall now examine the facts observed during the epidemic of dysentery in the *Laurel*, in order to ascertain which theory they support. The 49 steerage cases occurred in twenty families, and two were in unconnected individuals. The following table shows the number of members of each family, and the number attacked.

Family.	No. of members in the family.	No. attacked with dysentery.
1. Willis	9	4
2. E. Harrison	10	4
3. Allen	3	2
4. Adams	6	1
5. Standing	3	3
6. Honiss and P.	5	4
7. Pearce	3	1
8. Miller	11	3
9. Terry	2	1
10. Spray	6	4
11. W. Harrison	8	3
12. Phillpotts	7	4
13. Russell	7	1
14. Tucker	6	2
15. Bolton	4	3
16. Gilmour	3	1
17. French	2	1
18. Martin	8	3
19. Albury	2	1
20. King	2	1
Morrow & Bowman, two unconnected individuals	1	1
	109	49

After deducting Morrow and Bowman, there are 47 attacks in twenty families numbering 107 individuals.

The 217 steerage and 8 second-cabin passengers make 225; and deducting from this the 109 who were attacked and their families, there remain other 116 steerage passengers to be considered.

In these 116 persons, there was a family of ten members, two families each of seven persons, one of six, two of four, seven of three, eight families each of two persons, and 27 persons were unconnected individuals. When we consider that all the cases of dysentery appeared in twenty families and other two individuals, and that 21 families and other 27 separate persons were free from it, it appears to be correct to ascribe some of the cases to infection. As the diet of all was nearly uniform, and the chance of exposure to wet and of having damp beds was the same to all, we might have expected that the 49 cases would have been distributed more generally among all or nearly all the families, and also that the unconnected individuals would have suffered in as great a proportion as those accompanying their families: as the result was different, this may perhaps be ascribed to infection. As to the effect of diet and damp beds, many of the seamen who occupied the *house* on deck had the discomfort of damp-beds, they were much more exposed to cold and wet than the passengers, and their diet was about the same as theirs, and there were no cases of dysentery among them. Reasoning from this analogy, we have therefore less reason to ascribe to diet and damp, much of the dysentery that appeared among the steerage passengers.

I shall now examine if there was any difference in the prevalence of the dysentery in the different parts of the passengers' deck. These I shall here divide into six imaginary divisions: abaft of the after-hatch, A on the right (or starboard) side of the ship, and B on the larboard (or left side). This part was the

worst situated with regard to ventilation and light; in A division were seven cases of dysentery, and eight cases in B division. The next two divisions are between the after and the main hatches, and including all the berths opposite both hatches; C on the right, and D on the left side. This division was the most favourably situated as regards ventilation and light, and there were about thirty more passengers accommodated in it than in A and B; in C there were nine, and in D, eight dysenteric cases. being a much smaller proportionate number than in A and B. Before the main-hatch was the E division on the right, and F on the left. In E, there were about six persons fewer than in F, or A, or B, and in it there was only one dysenteric case. In F there were ten dysenteric cases; in it the first case (Mrs Willis, 15th April) appeared, and the next cases were in other two families occupying berths on either side of hers. At the front part of E and F, there was a close partition (*nauticé*, bulkhead) bounding the passengers' deck, and shutting out air and light from the fore-hatch. When the first case appeared, I caused this partition to be taken down, and put up with each second plank left out; this admitted air and light; beyond it were the beams over the hold. (For these hatchways there ought to be grating-hatches fitted as well as close-hatches). On the starboard (right) side of the passengers' deck, there were seventeen cases, and on the larboard (left) side, 29 cases. The sleeping-places were large enough to hold two adults, or four or five children; generally a large family had only two berths, with a bed in each; it was impracticable to separate a sick member from the others of the family, as berths and bedding were wanting. Some perhaps will consider that these particulars and peculiarities in the prevalence of the dysentery do not require to be explained by having recourse to the agency of infection.

As the first case (Mrs Willis, 15th April) did not arise from infection, so some of the others may not have arisen from it either. But I consider that *some* of the cases arose from infection, and for the following reasons, because,—1. The second and next few cases occurred in the berths next to the one where the first one occurred. 2. The complaint prevailed more on one side of the deck than on the other, and more in some parts than in others, and most in the worst ventilated parts (A and B). 3. It attacked a number of members of certain families, while about an equal number of families were exempt. 4. It attacked only two out of 29 unconnected individuals, thus prevailing less among them than among those who were accompanied by their families. 5. The cabin-passengers were exempt from it. 6. The crew, with nearly similar diet, and greater exposure to cold and to wet, both in their beds and otherwise, were totally exempt from dysentery.

Granting that *some* of the attacks arose from infection, we may next consider *how many* of them did so. This question is one difficult to answer, and it would be much easier to give it a dogmatic than a perfectly accurate reply. Perhaps one-half or three-fourths of the dysenteric attacks arose from infection, and the remainder from damp beds, exposure to cold and wet on deck, or from peculiarities in diet.

The ship was a very leaky one; the berths ought not to be placed close to the ship's side, as in the *Laurel*, but about a foot or so away from it; in this way much dampness is avoided.

Feigned Indisposition.—In a merchant ship in bad weather, as in the British Channel, the duty of the seamen becomes almost uninterrupted, and some are apt to feign indisposition; this is called by the sailors the "*Channel Fever.*"

When a merchant ship (which has fewer seamen than a naval vessel) gets into cold, damp, and uncomfortable weather, on the Banks of Newfoundland, if the weather be stormy, the duty of the men is very fatiguing and irksome; and if a sailor appear to the others to be complaining of illness without much apparent reason, they give it the name of the "*Banks' Fever.*" These practices are attempted almost solely in ships where there is no surgeon.

Deaths on the Voyage.—When at anchor at Grosse Isle, May 29, James Young, seaman, aged 30, fell from the fore-top-gallant-mast-head on to the deck, a height of about eighty feet. His head struck against the bulwark; the poor man uttered, oh! there was a comminuted fracture of the anterior half of the cranium, cerebral matter and blood being scattered all round where he fell; there was compound fracture of both arms, and of one leg. He had *gutta percha* soles to his shoes, and it was thought that these being slippery might have occasioned his fall. I was told that within the last five years, other two similar accidents occurred in the *Laurel*; a boy fell from aloft on deck and was killed on the spot; a seaman fell from a good way up the mainmast, down through the after-hatch, into the lower-deck, a height of about seventy feet; he fell on his breech; the captain was a witness of the accident, and to his surprise, the man got up as if nothing had happened; he felt no particular inconvenience that day, but did no duty, nor on the next day, when he felt sore and stiff; on the third day he returned to his duty as usual; it is likely that his fall was broken, by striking against various ropes in his descent.

Besides this death from accident, there were five deaths from disease,—all in children.

1. Sarah T., aged one year, died on the 3d of May of tabes mesenterica of eight months' duration. This child was weaned

when six months old. Mrs T. had another child which died of the same disease, at the age of eighteen months. There was no cow on board, but there was a scanty supply of preserved milk. On leaving London, the appearance of the child was such, that it was not likely it would live more than a few weeks. With the better diet obtainable only on land, the child might perhaps have lived a few weeks longer there.

2. Elizabeth T., aged fourteen months; May 12, was seized with dysentery, and on 15th the eruption of measles appeared. The berths of the family were opposite the main-hatch, and in a very exposed and cold situation. She died on the 19th May, principally from the dysentery.

3. Martha T., aged three years; May 8, was attacked with dysentery, and on the 10th the eruption of measles appeared. From the combined effect of the two diseases she sank on the 26th of May. In these two cases, ipecacuanha and also sinapisms to the chest were employed for the one complaint, and blisters to the abdomen, and acetate of lead and opium, and other remedies for the dysentery; and the strength was supported with small quantities of port during the last few days. Either the chest affection in measles or dysentery is much more easily treated when existing singly than when they occur at the same time, as the remedies requisite to check the discharge from the intestinal mucous membrane have a suppressive and injurious effect on the bronchial secretion: the indication is to direct most attention, in the use of internal remedies, to whichever of the two inflammations is the most violent; to employ diligently rubefacient or vesicatory applications to the chest and abdomen, and to carefully support the strength of the young patient.

4. Robert R., aged twelve months, was a very delicate child; and, after being somewhat reduced by diarrhœa, was seized with pneumonia, and died on the 12th of May.

5. John H., aged five years, has been frequently ill with diarrhœa, and several times with dysentery. This family, five in number, were intermediate passengers, and had a door to their berth, thus making their two bed-places more confined with regard to air: the other members of the family had dysentery before this boy was seized: he died on the 21st of May.

If the *Laurel* had had the average passage of forty days, she would have arrived on the 16th of May; and three of the five deaths occurred after the 16th of May.

On arriving at Grosse Isle quarantine station, there were no sick to be landed; and after remaining a few hours, the ship proceeded on to Quebec. During the whole passage, ventilating the passengers' deck was attended to as much as possible. In cold weather, and towards evening, using wind-sails may at times induce catarrhs or similar affections in persons confined to bed, and whose berths are near the mouth of the wind-sail. Sir William Burnett's disinfecting fluid, the solution of the chloride of zinc, was used once and sometimes twice daily: the disagreeable odour existing in the morning soon disappeared after it was sprinkled on the deck, and its use was productive of much comfort. I also think that it was of material service with regard to the health of the passengers, and that there would likely have been many more cases of dysentery, if it had not been used. (See the Edinburgh Med. and Surg. Journal for January 1848, and October 1848, and Dr Hall's Montreal Medical Journal for June 1848.)

As the law now stands, an emigrant ship to North America is not required to carry a surgeon, whatever the number of passengers be, if each passenger have fourteen superficial feet of space on the passengers' deck. But if each passenger have only twelve superficial feet, and if also, at the same time, there be more than a hundred passengers, then the ship must carry a surgeon. Though it be a great advantage to have two additional feet of space, still it is absurd to consider that this is equivalent to the advantage of having medical advice on board. The "Laurel" had 2800 superficial feet of accommodation for steerage passengers; (this is marked on a ship's *papers* by the government emigration agent.) Dividing 2800 superficial feet by 12 feet, gives 233 as the number of statute adults the ship can legally carry, provided there be a surgeon on board.

If there be no surgeon, then we have to divide 2800 superficial feet by 14 feet; and 200 adults is all that the vessel can legally carry.

The vessel can carry 33 more with a surgeon; and the passage-money of 33 persons at L.6, 5s. Sterling each, amounts to L.206, 5s.

We take an average passage of forty days; and allow a shilling a-day (for each passenger) as the cost to the owner of his provisions, and any fittings for the berths. This leaves L.4, 5s. to the owner for the simple conveyance of each passenger, or L.140, 5s. for 33 persons.

Supposing that the ship-owner engaged a surgeon, and was just or generous enough to pay him L.70, 5s., he would thereby be the means of himself gaining L.70 more than if there were no surgeon. If there had been no surgeon in the *Laurel*, I am nearly certain that two or three of the *adult* dysenteric cases would have ended fatally; and I think it likely that several other of the *adults* ill of dysentery would also have died. The estimate of a passenger's provisions costing a shilling a-day, is a liberal one for the owner, as, from the provisions being purchased in large quantities, and tea, sugar, &c. for sea-use being duty-free, the expense is less than a shilling a-day.

In order to guard against any error, I went over these calculations with the captain of the ship.

I have assumed that the 33 additional passengers are steerage ones, at L.6, 5s. each; but all might be intermediate passengers, at L.7, 5s. each; and a likely proportion is, that one-third would be intermediate passengers, thus making the advantage to the owner L.11 more than I have reckoned above.

With respect to forming an estimate of the proper sum for the surgeon's salary, it is to be considered that the passage out may last ten weeks. Allowing three weeks for the vessel to commence her passage home, and five weeks to accomplish this, makes eighteen weeks, or 126 days. It is here supposed that the surgeon has a free passage and mess out and home, and that bedding is provided by the ship. Ten shillings a-day for 126 days is L.63; and the duties ought not to be performed for less than this. A more satisfactory arrangement would be a certain fixed sum; and in addition, *so much* for each emigrant landed in America. As I stated in my former communication, there ought not to exist the exemption which there is at present for many emigrant ships from having to carry surgeons. Another change is also loudly called for, and it is this, that the salary of the surgeon be paid by the ship-owner or broker into the hands of the government emigration-agent, and from the latter the surgeon should receive it. The ship-owners, very frequently, are not fit to be trusted in the matter. As it is now, frequently the law is virtually evaded (by an owner engaging, for the sake of economy, non-medical persons as surgeons.) Professional men do not receive anything like adequate remuneration; and the public suffer in various ways from imposition.

In this communication, I have endeavoured to give a view of the diseases of emigrants on ship-board, and to notice some of the defects which still exist in the manner in which emigration is conducted. And when it is considered what a strong current of emigration is constantly flowing from the British isles to North America, the importance of these annual thousands and tens of thousands* of our fellow-countrymen being conveyed across the Atlantic with the greatest possible attention to their health and comfort, will not be lightly esteemed by the physician and the philanthropist.

Royal Marine Infirmary,
Chatham, September 1849.

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* Between May 1849 and the 6th of October 1849, thirty-eight thousand, one hundred and sixty-eight emigrants arrived at Quebec.

ART. III.—*On the* TRICHOPHYTON TONSURANS, *the Shaving Scall or Ringworm; a Contribution to the Comparative History of the Diseases which cause Baldness.* By P. H. MALMSTEN. From the Swedish. (Muller's Archiv, 1848, page 1.)

AT the meeting of Skandinavian physicians and naturalists at Christiania, I presented the following short treatise, which I am here, I believe, the first to communicate.

On the vegetable nature of the disease which Mahon describes under the name of Shaving Ringworm (*Squarus tondens*), and which is generally considered as identical with the *Porrigo decalvans* of Willan, with an attempt to determine the difference between these two diseases.

The *Squarus tondens* of Mahon, as well as the *Porrigo decalvans*, of Willan, are peculiar to the hairy portions of the head, and are generally considered as one and the same disease, though by different writers they are differently named. *Porrigo decalvans,* which is distinguished by round, bald spots, from which the hair has fallen, and which are covered with white powder, or small greyish scales, appears to have been well known to the ancient authors. Celsus calls it *Arca*, and describes two species of it, one of which corresponds to the *Alopekia* of the Greeks, the other to their *Ophiasis*. Sauvages calls it *Alopekia areata*; Good, *Trichiasis Area*. All these denominations have reference to the operation of the disease, namely, the formation of round, bald patches on the head.

Willan, who arranges the disease under the genus *Porrigo*,—although certainly improperly, since *Porrigo* denotes a pustular infectious cutaneous disease, while in that under consideration no pustules or any similar eruption take place,—gives it the name of *Porrigo decalvans*. Bateman, whose fortieth plate represents the *Porrigo decalvans*, namely, round, bald, smooth, shining patches, here and there on the scalp, describes it in the following terms:—" This singular variety of the disease presents no appearance whatever, except patches of simple baldness, of a more or less circular form, on which not a single hair remains, while that which surrounds the patches is as thick as usual. The surface of the scalp within these *areæ* is smooth, shining, and remarkably white. It is probable, though not ascertained, that there may be an eruption of minute achores about the roots of the hair, in the first instance, which are not permanent, and do not discharge any fluid."* And in the explanation of the same engraving, he adds, " *Porrigo decalvans;* a variety also named ringworm, and which sometimes is a consequence of the decalvating process which the foregoing species (*Porrigo scutulata*) produces."

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* A Practical Synopsis of Cutaneous Diseases, &c. By Thomas Bateman. Fifth edition, London, 1819, p. 175.

This last, namely, *Porrigo scutulata*, the author professes to represent in the thirty-ninth plate. But it must, in my opinion, be impossible to represent this disease (the shield-shaped scall of the Germans, the nummular ringworm of the French), which properly is a variety of *Porrigo favosa*, *Favus*, malignant scall, bald scall, the *Porrigophyta* of Gruby, and is in general considered as such in the present day. This plate shows further, much resemblance to the delineation which Mahon has given of the shaving Tinea (*Teigne tondante*) in his researches on the seat and nature of the *Tineæ*.*

From Bateman's description of the *Porrigo scutulata* it evidently results, that, while he attached too much importance to the circular shape of the spots and the contagious nature of the disease, he has confounded two different diseases, namely, the variety of the bad scall which I have above mentioned, and which is usually, by reason of its shape, called *Porrigo scutulata*, *Favus scutiformus*, and the disease which Mahon first adequately discribed under the name of *Teigne tondante*. Bateman, in his description of the *Porrigo scutulata*, makes, among other statements, the following:—"Sometimes the achores are at the commencement of the disease scarcely visible; and the shedding of the hair is the first symptom of the presence of the disease." So also, from Willan and Bateman's description of the *Porrigo decalvans*, it seems probable that they have referred also to this head various examples of the *Teigne tondante* of Mahon.

Under the latter denomination, Mahon, who with his brother had occasion to observe a considerable number of eruptions on the scalp, has described a disease which in many respects approaches closely to the *Porrigo decalvans* of Willan, and has been by most authors considered to be the same disease, yet appears to be essentially different from it. I may be allowed to cite the following from the description given by Mahon:—

"This scall is recognized by the formation of small rough points on rounded spots, more or less extensive, usually on the hairy scalp. These rough points may be compared to those of the substance called chagrin, or the skin of the shark. The hairs which cover these spots are broken at the height of one or two lines above the epidermis, so that from them result real tonsures.

"The individuals affected by this scall have always presented, on the hairy scalp at least, a tonsure or bald spot, more or less extensive, but always regularly circular, on which the hairs were naturally cut or rather broken off at the distance of one or two lines above the level of the scarf-skin. At this place the skin was extremely dry, more compact, more dense than on the neighbouring sound parts; the rough points that offered themselves to observation were sensible to sight, and especially to touch; they were like those which became manifest on the surface of the skin in consequence of the sudden impression of cold, or after the shivering caused by a sensation of horror; in short, like what is commonly called *cutis anserina*, or goose-skin. The tint of the skin was a little bluish; but when it was rubbed, the surface subjected to this friction was covered by a fine very white powder, which might be compared to fine flour.

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* Recherches sur la Siege et la Nature des Teignes. Paris, 1829.

"The disease begins by showing itself on a very small point, which becomes the centre of a circle, which continues progressively to enlarge its circumference; the breaking of the hairs is the result of this change in the follicles. Sometimes the affection is communicated to other places distant from the seat of the first attack; there is established a minute point, which is developed by eccentric extension; at length all these circumferences terminate in uniting, in being confounded, and in forming of the whole head a surface entirely bald, and covered by the rough points already mentioned. We have seen three or four instances of the general attack of the hairy scalp by this singular affection."

Mahon,—who communicates some interesting examples of the disease, to which I refer, and saw the disease in several persons of one and the same family, found it contagious, very difficult to heal, and, in inveterate cases, as the malignant scall, the nails also affected,—regards it as closely interchangeable with *Favus*, and thinks that, like this, it has its proper seat in the sebaceous follicles. Good describes, under the name of *Porrigo circinnata,* a disease analogous to the *Teigne tondante* of Mahon. Alibert, who considers Mahon's *Teigne tondante* as quite identical with Willan's *Porrigo decalvans,* and with justice praises highly the description given by Mahon, calls it *Porrigo tonsoria,* but does not adopt Mahon's opinion that it alternates with *Favus,* and cautions against its being confounded with a disease of Favose character, which is distinguished only by the falling of the hair;—*Favus sine Favis,* which, according to his statement, he has observed, and respecting which, without more minute description, he observes:—This is called baldness (*calvitium*), the name being derived from the part affected; for the anterior part of the head, formerly denominated *calva,* becomes smooth.*

. Rayer, who evidently considers the *Porrigo decalvans* of Willan, and the *Teigne tondante* of Mahon, as very closely allied, yet not one and the same disease, mentions both under the article *Alopekia,* and states that he has observed instances of the former disease in grown persons as well as in children, but only one single instance of the latter affection in a child. How rare a disorder, also, is the *Teigne tondante* described by Mahon, may be inferred from the calculation made by this observer, at a time when he had in the course of several years already treated more than 4,000 cases of scalled head. This calculation shows that, among 100 cases of scalled head, 75 were of *Tinea favosa;* 11 of *Tinea granulata;* 7 of *Tinea mucosa;* and 6 of *Tinea furfuracea;* while, on the other hand, *Tinea tondans* occurred only from two to three times in one thousand instances. Subsequent to this period, Dr Gillette described, in the Gazette Medicale for 1839, p. 573, a form of contagious Alopekia, which appears fully to agree with the *Porrigo decalvans* of Willan. While, in consequence of these circumstances, observers have begun in recent times, on good grounds, to refer to their proper orders the different cutaneous diseases taking place on the hairy scalp, known by the name of Scalls, and have therefore described an *Ekzema Capitis*, an *Impetigo Capitis*, and similar affections,—we find also described by authors a *Pityriasis decalvans* (Gibert), and a *Herpes tonsurans* (Cazenave), which last perfectly agrees with the *Tinea tondens* of Mahon.

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* "Dictum autem calvitium est, sumpto nomine a parte affecta; nam glabrescit anterior capitis pars, calva antiquiter nuncupata."

On the 14th of August 1843, Dr Gruby communicated to the Academy of Sciences in Paris, his Researches on a New Species of Cryptogamous Plants, which constitutes a contagious disease of the skin, named *Porrigo decalvans.* According to the researches of this author, the observer finds, by means of the microscope, in the white powder which covers the skin, on the bald circular spots which causes *Porrigo decalvans* on the head, kryptogamous plants, which also in large numbers surround the hairs attacked by the disease, and form a sort of vegetable sheath or enclosure, which surrounds the hair from its emergence from the skin to from one to three millimetres' distance from the scalp. This vegetable sheath is something enlarged at the point where it is in contact with the scarf-skin, and has its roots in the superficial epidermal cells. It is in such firm connection with the hairs, that the latter break before it is detached. By means of the microscope, the observer learns, that the sheath consists of kryptogamous plants, which are closely connected with each other, and, at the same time, form a tube round each hair. This vegetable covering consists of small stems, branches, and *sporulæ.* The stalks arise in the texture of the hair, and form the internal layer of the investment, while the *sporulæ* are deposited outside. The thickness of the wall of investment amounts to $\frac{15}{1000}$ parts of a millimetre. The branches run in an undulating and serpentine course, follow the direction of the hair-fibres, are transparent, and from $\frac{2}{1000}$ to $\frac{3}{1000}$ parts of a millimetre in diameter. They contain no molecules. The stems are as thick as the branches, which become visible through the accompanying sporules. The branches terminate at the outer surface of the sheath, which is entirely covered with sporules. These are in general deposited closely below each other at the outer surface of the sheath; but the observer discovers also, on the hairs themselves, individual sporules, which are firmly attached to the stems. These sporules are mostly round, sometimes, however, oval, transparent, not molecular, and swell when they come in contact with water. The round sporules are from $\frac{1}{1000}$ to $\frac{5}{1000}$ parts of a millimetre in diameter; the oval sporules are from $\frac{2}{1000}$ to $\frac{5}{1000}$ parts of a millimetre in the short diameter, and from $\frac{4}{1000}$ to $\frac{8}{1000}$ parts in the long diameter. This parasitical plant Gruby names from the small sporules, *microsporum*, and, in remembrance of Audouin, on account of his observations on the muscardine in the silk-worm caterpillar,* *microsporum Audouini.* This *microsporum Audouini* arises first at the surface of the hair, one or two millimetres distant from the epidermis. The hair is at this spot opake; there spring thence molecules so minute that they can scarcely be measured, from $\frac{1}{10000}$ to $\frac{2}{10000}$ parts of a millimetre. At this part are observed the first traces of the *microsporum*, which afterwards spreads to the contiguous hairs, while these are progressively changed until they fall off in pieces, and baldness follows.

The structure of the hair undergoes many changes from the presence of the parasites, the roots of which are on the surface of the hairs. It is at first dark, and its previously smooth surface is furrowed. The epithelium loses its lustre and its cohesion, and drops off. The hair is brittle, breaks by merely bending it, and at length drops off. The hairless patches are then grey-white, while still, on the surface of the epidermis, is left a number of kryptogamous plants, the cells of which become in like manner a seat of the disease. Excepting the *kryptogama* here described, the observer finds no other pathological phenomenon, no trace of inflammation, no vesicles or pustules, no hypertrophy of the epidermis. Gruby accordingly names the disease, in consequence of its nature and effects, PHYTOALOPEKIA.

These kryptogamous productions are developed, and increase with incredible rapidity. It requires only one part of the skin to come in contact with them, in order to cover, with their parasitical vegetation, within the space of a few days, patches of three or four centimetres. The hairs are grey at their first issuing from the scalp, and already, within eight days, they break where they are surrounded by the *kryptogama.* The thick ones resist longer. Around them the parasites are accumulated often to so great amount, that they form small greyish elevations, which have been erroneously regarded as vesicles or pustules, or a secretion from the hair-follicles.

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* Muscardine is a disease liable to attack silk-worms, and it consists in the formation and growth of a very minute microscopical parasitical plant in the bodies of these animals.

In the middle of February 1844, I had occasion, shortly after I had become acquainted with the researches of Gruby, to observe, for the first time, an example of disease, which perfectly corresponded with Mahon's description of the *Tinea tondans*. I watched since that time this case with sufficient attention, and shall now shortly state the results to which adequate and oft-repeated microscopical inspection have conducted me.

Axel H., in July 1844, two years and eight months of age, was sprung of healthy parents, and was nursed by his mother with the greatest attention. He had never been unwell. The complexion was fresh and ruddy; he had no mark of scrofula, and had at no time been attacked by any eruption. In November 1843, the mother observed, when she combed the child's hair and washed his head, about one inch from the large fontenelle, on the right side, a small patch covered with grey-white scales, on which the hair had disappeared. At this she wondered; washed the child's head diligently, and combed out the scales from the small patch; but they returned notwithstanding, and the woman soon observed that the patch was gradually enlarging. As this appearance continued, she resolved, in February 1844, to consult a physician.

When I first saw the boy, the circular patch was one inch and one-third in diameter, covered with grey-white scales, from which, on closer inspection, appeared to issue a quantity of minute hairs, about two lines long, which were of clear colour, and without lustre. The patch was dry, rough to the feeling, and its colour passing into blue-grey. When the scales were carefully scraped off, the skin beneath them appeared to be quite uninjured. The hairs surrounding the patch were unaltered. A little higher up backward, and nearer the vertex, was another smaller patch two lines in diameter, from which the hairs, at the distance of about two lines from the skin, were in like manner cut off, and the skin itself presented the papillated aspect of goose-skin, (*cutis anserina*); while around the hairs, where they emerged from the skin, there stood spaces like small elevations of scales,—an appearance which probably is the small *achores* mentioned by Bateman. When the hair of the head was entirely cut away quite short, so that it was not more than a quarter of an inch in length, there appeared on several spots, dispersed here and there over the head, very small, scaly, slightly-elevated patches, from which the hair appeared not yet to have dropped away, although we found, on more attentive inspection, both sorts of hair equally cut off.[*]

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[*] In this instance, the author employs the term cut off to denote the kind of cutting or destruction which is effected by the disease, not by instruments.

After we had allowed the hair to grow long for some time, we saw arise or spring upright from the hair lying previously smooth, one or two hairs; and these hairs disappeared with unwonted facility; but when they were about two lines distant from the scalp, all of them were bent at an angle, that is, there was here something like a joint in the hair. These hairs were most numerous at the edge of the large patches, and on the right of the small scaly patches. On the 7th of May, I mentioned to the Society of Swedish Physicians in Stockholm, this instance of cutaneous disease, and its vegetable nature; and when, in the beginning of July, I was writing this report, the large circular patch had spread to so great an extent, that it was two inches in diameter, and the smallest was about half an inch in diameter.

When we cautiously pluck from the projecting scales, in this stage of the disease, residual fragments of hairs, from one to two lines long, we find, with the aid of the microscope, magnifying with a linear power of three hundred time, these ai r-fragments internally perfectly filled with sporules between the fibres of the hair. Should it happen, on pulling out the hair, that its root follows with it,—which, in the meantime, in consequence of the brittleness of the hair, is very difficult,—the observer may perceive that already, in the root of the hair itself, the formation of the disease has commenced, and how the sporules are occasionally deposited in the manner of a chaplet or rosary, and even, in particular instances, branches come out divided. After I had endeavoured, by means of eye-lash forceps, to extract all the similar hair fragments from the large patch, and this patch had thus become almost entirely bald, there arose still, after the lapse of some days, a quantity of similar hairs, which were in the same manner attacked with the scall-disease. This circumstance proves that the first origin of the scall begins in the root of the hair. In the scalls which cover the patch, we find, by microscopical inspection, between the epidermal cells, a quantity of hair fragments, which in like manner are loaded with sporules between their fibres, although they are variously arched and twisted. It is probable that the colour of the patch, inclining somewhat to blue-grey, is derived from these fragments, which lie mingled with the epidermal cells. The sporules are round, transparent, not molecular, and present a diameter usually of $\frac{5}{1000}$ of a millimetre; for the sake of comparison, it might be said that they are about half as large as the blood-granules of man. When we examine the hairs already mentioned as arched, prominent, and easily dropping out, their bulbs appear at same time withered, almost always bent, and the root is surrounded by a net-work, which, though changed by disease, is probably part of the natural healthy investment. A net-work of this kind may be observed on the hairs of different animals; but in those of man it is not seen in the normal state, but appears to belong to a morbid condition, as I have sometimes had occasion to observe a similar net-work, in which the hairs had dropped out from another cause than the disease under consideration. What connection, in other respects, this reticular formation round the roots of the hair may have with the production of this species of scall, I leave to be the subject of subsequent consideration.

From the facts now adduced we clearly perceive the vegetable nature of this disease,—that is, that it arises from the presence of a kryptogamous plant, which grows within the hair and renders it brittle, so that it is easily broken; we find also that the scall under consideration is entirely different from that which Gruby found and described in the *Porrigo decalvans*; and as the case observed by me fully agrees in external characters with Mahon's *Teigne tondante* (*Squarus tondens*), so it appears very probable, that Willan's *Porrigo decalvans*, and Mahon's *Teigne tondante*, are two perfectly distinct diseases, which arise from two different scalls, the first of which agrees with the disease described by Gruby, and the second with the one now made known by the author of this paper. Subsequent researches and observations may in the meantime give precision to this conjecture of mine. While Gruby gives the name of PHYTOALOPEKIA to *Porrigo decalvans*,—the vegetable nature of which he has made known and described,—so may we, with particular reference to the circumstance, that, in the instance of cutaneous disorder witnessed by me, the formation of the scall begins in the root of the hair, apply in consequence to this disease the name of RHIZOPHYTOALOPEKIA.

As an appendix to the instance communicated by me, I have still to add, that the mother of the child, upon my interrogation whether any cause of the origin of the disease was known to her, repeated that she knew of no other than this, that the child might probably have been infected by a young girl, who during the summer of 1843 had acted to him as child's nurse, and on whose head she observed a round bald patch, which the girl always had endeavoured carefully to conceal beneath her head-dress.

As to the determination of the *Genus fungornm*, to which the form of Baldness described by me belongs, I leave it to the Scalls. To me it appears to possess much resemblance to the *Torula olivacea* or the *Torula abbreviata* (Corda).

The point which I submitted in the above chapter as probable, and referred to further observations to determine, namely, how far Willan's *Porrigo decalvans*, and Mahon's *Teigne tendante*, are two distinct affections which arise from two different sorts of scall, has since been fully established by the observations of others, and by my own. In the spring of last year I transmitted to Dr

Gruby at Paris, hair from the head of the child whose case is given, and furnished to him a report upon the course which I had observed in that case, chiefly in order to obtain from him the therapeutic counsel, which had cured with such remarkable success the most obdurate cases of *Tinea favosa*, the malignant scall (*Porrigophyta Schimmelgrind.*) In his answer, he informs me,—"Your letter and the accompanying hairs are to me highly interesting. I have already had occasion to observe these two species, the PHYTOALOPEKIA and the RHIZOPHYTOALOPEKIA, and for a year have made attempts to cure the disease; but hitherto these attempts have failed." And in the Gazette Medicale, No. 14, 1844, which number I first had the opportunity of seeing in the previous harvest (1847);—I found that Gruby had presented to the Academy of Sciences at Paris, on the first of April, a memoir entitled Researches on the *Kryptogama*, which constitute the contagious disease of the scalp, described under the name of *Teigne tondante* (*Herpes tonsurans*). From the short extract which is given in the number now mentioned, the observations made by Gruby and myself, both independently of each other, appear fully to agree.

I have further had occasion, during the year now elapsed, to observe five more instances of the disease under consideration; and in all these I have found confirmed the statements which I made regarding the nature of the disease in the above communicated and first observed case. From the experience obtained from these five instances, as well as from the observations of Gruby, it is now placed beyond all doubt that the *Porrigo decalvans* of Willan, and the *Teigne tondante* of Mahon, are two different diseases, which, indeed both proceed from the formation of a bald scall, but which are entirely different in the diseases under consideration. The principal differences I shall shortly specify.

1. In the *Teigne tondante*, the baldness arises in the root of the hair, and comes on the hair only between its fibres, so that the epithelial covering of the hair remains uninjured. Besides, we find no trace of scall-formation between the epidermal cells, so that in this instance, it may be justly said that the disease belongs exclusively to the hair, On the other hand, in *Porrigo decalvans* it is exactly the external part of the hair, namely, its epidermal covering, which is attacked by the scall-formation, which forms round the hair a sheath, which at the same time destroys the hair; and the scall-formation taking place here, advances also between the epidermal cells, belongs accordingly not exclusively to the hair.

2. The *Kryptogama* themselves are also different. In the *Teigne tondante* the scall-formation almost exclusively presents sporules,

and this is very rarely elongated, deposited in the chaplet-form, so that they resemble divided branches; the sporules are larger in size, from $\frac{5}{1000}$ to $\frac{8}{1000}$ parts of a millimetere. On the other hand, the scall-formation in *Porrigo decalvans* has stems and numerous serpentine branches with sporules by their sides. These sporules are much smaller in size than those of the *Teigne tondante*, being from $\frac{1}{1000}$ to $\frac{5}{1000}$ of a millimetere.

Gruby, as was already mentioned, denominates the *Porrigo decalvans*, PHYTOALOPEKIA; and, in accordance with this, the *Teigne tondante* is named RHIZOPHYTOALOPEKIA. The last I have called shaving-scall (Harskarande Mogel, Swedish; Haarscherender Schimmel, German); and in Latin I might call it *Trichophyton tonsurans*, or *Trichomyces tonsurans*. The first denomination I prefer; as Gruby, it is well known, has named the malignant scall PORRIGOPHYTA, and a species of *Mentagra*, which depends only on a scall, MENTAGROPHYTA. The *Porrigo decalvans* might be named TRICHOPHYTON, or TRICHOMYCES DECALVANS, provided it be wished to preserve old characteristic and convenient epithets.

It would be most proper to endeavour to express in the denominations of both diseases their most characteristic features, namely, that in the one the scall-formation takes place within the hair, while in the other it is on the exterior of the hair; but such denominations might be inconvenient.

As to the external signs of the TRICHOPHYTON TONSURANS, I have, in the six cases which I had occasion to observe, found the habit of the disease as it has been described by Mahon. I shall only advert to the difference which the disease naturally shows in those cases in which the head is kept very clean; because it especially happens, when the disease is not disturbed by washing and combing, that it shows patches resembling shark's skin; but, when it is attempted steadily to comb away the scales, often beneath there appears slight redness, and from the irritation occasionally small pustules or crusts are formed. I have accordingly, in one of the several cases, seen the disease complicated with an impetiginous eruption, or rather become so complicated during treatment. But, when the disease proceeds without any such complication, and particularly before treatment has been commenced, we find on the spots attacked by it only an increased formation of scales, and the bodies like broken fragments of hair projecting from the scales. The disease in this case closely resembles a variety of *Pityriasis*, from which, nevertheless, it may in most cases be easily distinguished, even without the aid of microscopical examination. In the ordinary form of *Pityriasis capitis*, the hair does not fall off; on the contrary, this disease often takes place in females who have an unusually thick growth of hair,

—a circumstance which renders very difficult the cure of the disorder. In TRICHOPHYTON TONSURANS, again, we see how the hair from the scaly patches is broken and drops off; and, when the scales are rubbed off, we find between them a quantity of small fragments of hair. With *Herpes*, again, TRICHOPHYTON TONSURANS can never be confounded; because it in common with it nothing possesses but the circular shape, and no vesicles appear, except secondarily from irritation, which care and management are adequate to prevent.

The distemper appears to happen especially to children. Of the six cases which I have witnessed, one only took place in a grown person; and, in this instance also, the patient, a girl of 20 years, had, according to her own statement, suffered under the complaint from childhood.

As to the causes of the disease, I have, in five of those six cases, ample reason to ascribe the origin of the disorder to contagion; in two instances from child's maids, who had attended the respective patients. This circumstance appears to demand particular attention to be paid in the selection of a nurse or child's attendant. The children were otherwise healthy, and, if I except one, not in any way scrofulous; and all were carefully attended to.

It must be mentioned, however, that in some cases the growth of the hair appeared for some time previous to the outbreak of the disease to be a little unhealthy, and the hair was dry,—a circumstance which appears to indicate that the scall-formation does not readily take place without previous morbidity, or a morbid disposition in the hair. In the last five cases, I did not observe round the root of the hair the network which I described in the case first communicated; so that this phenomenon may probably be in some accidental manner connected with the disease. In what sense the disease may arise spontaneously,—that is, through what is called equivocal generation,—I am unwilling to consider; but this is accounted not impossible, since the scall under consideration actually belongs to the lowest vegetable formations; and the possibility of such origin is not contradicted by the power which the disease possesses of being propagated also by contagion.

Lastly, as to the treatment of this distemper;—while Mahon complains of the obstinacy of the affection, he nevertheless adds, —"By assiduous cares we have always got the better of it, by employing against it the same means which *tinea favosa* is unable to resist." Celsus, in reference to the treatment of the disorder, which he names *area*, expresses himself in the following terms:— "Some aggravate the state of these patches by the knife. Some anoint them with caustic applications in oil, and principally burnt paper. Some apply turpentine resine with thapsia. But nothing is better than daily shaving the surface by the razor; because, when the superficial pellicle is gradually removed, the roots of the hairs are opened up. Nor ought this method of treatment to be stopped until it appears that the hair is growing thick. Further, the space which is habitually shaven it is sufficient to paint with shoemakers' ink."*

In regard to the disease termed by Willan and Bateman *Porrigo decalvans*, the latter writer expresses himself in the following manner:—"If the scalp is cleared by constant shaving, and at the same time some stimulant liniment be steadily applied to it, this obstinate affection may be at length overcome, and the hair will regain its usual strength and colour." And he then recommends, as most efficacious, friction with a liniment containing some essential oil dissolved in spirit; for instance, two drachms of oil of mace in three or four ounces of alkohol, or with liniments prepared with oil of tar, Barbadoes rock pitch, camphor, or turpentine.

The experience which I have acquired with regard to the treatment of *Trichophyton tonsurans*, I shall briefly state. The case first observed I treated for the space of half a year, in the following manner. I caused the head of the child at first to be several times shaved, and employed for a long time over the patches pencilling with iodine tincture. By the irritation produced in this manner, there were formed on the patches lamellar scales, which I then endeavoured cautiously to remove. In this way I proposed, by means of the iodine, to destroy the scall; and there followed besides, with the removed lamellar scales, numerous morbid fragments of hair. As this result, however, was not accomplished, I employed touchings with nitrate of silver, though also without success. I next caused for a long time embrocations with impure wood-vinegar, and afterwards with kreosote, to be applied over the child's head. But as this also appeared to produce little favourable change, I at length directed the mother to keep the head of the child perfectly clean, to wash it daily with soap and water, and to comb the hair assiduously. I saw the child again after a long interval; and though then he was only in a better state, at present the disease is decidedly abated. The small patches are with slight exceptions gone, and upon the large ones, the circumference of which is greatly diminished, hair is growing, though thinly.

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* "Quidam haec genera arearum scalpello exasperant; quidam illinunt adurentia ex oleo, maximeque chartam combustam; quidam resinam terebinthinam cum thapsia inducunt. Sed nihil melius est quam novacula quotidie radere: quia cum paullatim summa pellicula excisa est, adaperiuntur pilorum rediculae. Neque ante oportet desistere, quam frequentum pilum nasci apparuerit. Id autem, quod subinde raditur, illini atramento sutorio satis est."

The term *Atramentum Sutorium* is employed both by Celsus and Pliny, to signify a staining liquid employed by the ancient Roman shoemakers, and which consisted chiefly of a solution of sulphate of iron; often from that circumstance called Roman Vitriol.

In another case, in a boy who is at present two years and a half old, and in whom the disease appeared the previous spring, I employed at first frictions with a liniment, consisting of the essential oil of cloves, and petroleum in the proportion of two drachms of the former, and four ounces of the latter article, but only for some weeks, as the liniment was powerfully irritating. After that, washing with water and soap were the principal means; and under their use the disease is at present almost gone.

Since the period of those trials, I have in two cases recommended merely combing and washing the head of the child assiduously; in short, to bestow great attention on the hair, upon the principle, that as the growth of the hair is improved, the disease also progressively disappears.

The prognosis is consequently in general good, provided the disease is not permitted, from negligence and want of cleanliness, to make progress, in which case it is liable to induce baldness, and, according to the observation of Mahon, it may also attack the nails. Should the disease take place in a scrofulous child, or be complicated with an eruption on the head, the treatment must be directed against these circumstances.

ART. IV.—*Successful Case of Cæsarean Section, with Remarks.* By THOMAS RADFORD, M.D., F.R.C.P. Edin., Consulting Physician to the Manchester Lying-in Hospital, &c. (Read at the Anniversary Meeting of the Provincial Medical and Surgical Association, held at Worcester, August 1st, 1849.)

THE subject of this case—Mary, wife of William Haigh—resides at Flats Fold, a mile from Ashton-under-Lyne, and about eight miles from Manchester. On my arrival, at half-past three o'clock P.M., I called upon Mr Cluley, who accompanied me to the case, and, with the greatest courtesy and candour, gave me as we passed along the following particulars:—She had felt slight pains, according to the account of the friends, about a week; but Mr Cluley thought that true parturient pains had only existed about three days, and which were so slight as not to require his interference. On this day (Sunday, May 20th, 1849), at nine o'clock, he was again called; and, although the pains were still trifling, he made an examination *per vaginam*, but was unable to feel either the *os uteri*, or the presentation; he therefore had her taken out of bed and placed on the lap of a female friend, and again repeated his inquiry. The head of the infant was now felt, and the *os uteri* found dilated to the size of a half-crown piece. In this manœuvre he unintentionally ruptured the membranes. The pelvis, he mentioned, was considerably contracted. I found her lying on the right side. Pulse 120; tongue clean and moist; her countenance tranquil, but a little flushed. Her bowels had been freely and fully moved this morning; and she had also freely and duly urinated. She was helplessly fixed on her side, and, when requested to turn, she remarked that she suffered very great pain when she made an attempt to do so, or was by another person turned on the back. The pelvis was very considerably altered from its natural shape: its sides were flatter; and the posterior division of the ilia, especially on the left side, projected backwards; and the upper portion of the sacrum, and the lower lumbar vertebræ, had sunk in an inward and downward direction, so that a great concavity was perceived here. The uterus inclined rather to the right side, and stood considerably more forward than usual, although it had not assumed the retort form to the same degree as I have witnessed in former cases; its tissue felt soft and compressible. The fundus or upper division of the organ was fluctuant, and rounder in shape than it generally is after the discharge of the *liquor amnii*, which led me to conclude that a great portion of this fluid still remained. This opinion was corroborated when I attempted to ascertain the position of the infant through the abdominal parietes; for, at the lower or cervical portion of the uterus, from whence it was presumed the fluid had escaped, the projections of its body could only be felt.

By a vaginal examination I found the lower aperture of the pelvis very considerably diminished by the close approximation of the rami of the ischia and pubes, which nearly destroyed the arch, and by their jutting forward there remained only a narrow slit, which would not admit the point of the finger. In the transverse diameter, two fingers could only just be placed between the *tubera ischii*, the antero-posterior diameter was also much shortened by the coccyx and the lower part of the sacrum being considerably incurvated. This great diminution in the outlet rendered it difficult to measure the brim, so that it was necessary to carry the hand very far backwards to accomplish it. Its figure was tripartite, or composed of three divisions. This alteration in the brim was occasioned by the falling downwards and forwards of the upper part of the sacrum, and the lower lumbar vertebræ, which inclined a little more to the left side, and by the body of the *ossa pubis* and ischii being forced backwards and inwards, and by the jutting forwards of the symphsis and rami of the pubis. The measurement of the widest part of the conjugate diameter, in the two lateral divisions, did not exceed an inch and a half; I could only place two fingers, one lying a little over the other. The anterior division was not more than half an inch in its widest part, as it would scarcely admit one finger edgeways. The length of this narrow opening is not relatively available in practice. In the transverse diameter of the brim I could just place three fingers, parallel with each other. The external genitals were free from tumefaction, and the vaginal lining was moist, and of a natural temperature. Whilst lying on her side I was unable to feel either the *os uteri* or the presenting part of the infant, but on placing her on her back (which occasioned her great pain), the os was felt to be dilated to rather more than the size of a shilling. She had not felt the movement of the infant since the morning, but by the stethoscope I satisfactorily heard the pulsations of its heart, which fact Mr Cluley afterwards corroborated.

With my opinion as to the position the Cæsarean section ought to take in obstetrics, I at once concluded that it was the only operation which was justifiable, and indeed capable of giving the best chance of life to both mother and infant. In this opinion Mr Cluley cordially acquiesced. We now informed her husband of the nature of the case, and the means to be adopted. He answered, "if nothing else would save her;" he willingly submitted to any plan we considered right. When a similar communication was made to the patient, she received it with the greatest resignation; it was unaccompanied by either mental or physical disturbance.

At Mr Cluley's request, Dr Lees, Messrs Hunt, Gibbons, Galt, and Brewster, were present at the operation.

Before the incision was made, I was anxious as far as possible to ascertain where the placenta was located, and I therefore placed my ear over the left division of the uterus. From the negative evidence, I concluded it was not fixed on this side of that organ. Mr Cluley adopted the same plan, but thought he heard the placental "soufflet." I again applied my ear, and still heard nothing. Dr Lees tried, and considered the sound to arise from the friction of the ear on an interposed piece of lint. Mr Cluley, after a second trial, agreed in my opinion. I therefore suggested the left side of the *linea alba* as the proper situation to make the incision. I now raised the *fundus uteri*, and Mr Cluley divided the abdominal integuments on the left side of the umbilicus, to about six inches in extent, from which very little blood was lost. An opening was now made into the uterus by a scapel, which was further extended upwards and downwards by the probe-pointed bistoury. At this stage some little bleeding took place from the divided sinuses; and there was also a considerable discharge of *liquor amnii.* I now, as quickly as possible, introduced one hand into the the uterus, over the infant's hip, and fixed the fingers under the flexed thigh in the groin, and having placed the other hand on the opposite side of its breach, I extracted it vigorously alive. During this manœuvre the uterus strongly and regularly contracted. The funis was now tied and divided by one of the gentlemen present. Afterwards I seized the funis with one hand, and with the other readily detached and brought away the placenta, which was fixed on the right latero-posterior surface of the uterus. There was some blood discharged, but not more than frequently happens after ordinary or natural labour. Several convolutions of intestines, with a portion of omentum, now protruded, which had up to this time been supported and effectually restrained under the abdominal parietes; but they were readily returned. I carried my finger round the wound to ascertain if any portion of these viscera had descended into the uterus.

The integuments were brought into proximity, and held together for a short time by a hand placed on each side; and, as there was no further discharge of blood, ligatures were inserted at an inch distance from each other. For this purpose, Mr Cluley used a long needle, with a scalpel-like handle, which admirably answered. It is much superior to those in ordinary use. Straps of adhesive plaster were laid across the wound, and on each side was placed a compress of lint, and over all a bandage, just tight enough to give firm support.

During the whole time the mind of the patient was calm, she never even uttered a complaint. She remarked that her sufferings during the operation had been much less than what she had endured previous to it. Pulse from 80 to 90 in the minute.

A draught, containing one drachm and a half of tincture of opium, was administered.

Half-past seven.—Pulse 100 to 120; dozing; there was no hæmorrhage or vomiting; had taken some gruel. Ordered mucilaginous beverages and farinaceous diet. At a later hour the same evening Mr Cluley saw her, and found a little abdominal uneasiness. She had slept, and had a lochial discharge.

May 21st, Monday, half-past two P.M.—Pulse 130; tongue moist; face less flushed; abdomen tympanitic, and slightly painful; fresh and plentiful lochia; bowels not moved; five ounces of water drawn by catheter.

Continue mucilaginous drinks, &c. An enema of warm water to be administered in the morning.

22d, Tuesday, half-past nine A.M.—Mr Cluley had ordered forty drops of tincture of opium, to be taken at bed-time. She had several times vomited a dark-coloured fluid during the night, and she still continues to do so; pulse 120; abdomen tympanitic, but not tender; tongue slightly furred; lochia natural; bowels still unmoved. After loosening the bandage, there was a discharge of sanious matter.

Ordered to have an enema, with one ounce of castor oil, and two ounces of oil of turpentine. To take likewise ten grains of compound extract of colocynth, with two grains of calomel.

23d, Wednesday, half-past nine A.M.—Hiccough has been troublesome; has bilious vomiting; tongue brownish; pulse 120; has a burning sensation in the throat, and the side of her mouth is excoriated; she says she tastes the turpentine which was given in the enema; bowels not moved.

Two drachms of sub-carbonate of soda to be dissolved in three ounces of distilled water and three ounces of mucilage, and the patient to take one ounce every third hour. Gum water to drink. To have an enema, with three ounces of ox-gall and one pint of water.

24th, Thursday, half-past nine A.M.—Symptoms continue the same, but the tongue is slightly aphthous; two enemata were administered, which produced two small scybalous stools; wound much lessened in size, but its edges are flabby and have not united; ligatures still firm.

To continue the same plan; to have the ox-gall enema repeated.

25th, Friday, half-past nine.—All the symptoms better; has had free alvine evacuation. The integuments over the sacrum are inflamed and excoriated, and have a tendency to slough. To continue the same means; ordered warm water enema. The parts over the sacrum to be dressed with collodion; warm water to be injected *per vaginam* into the uterus.

26th, Saturday, half-past nine A.M.—Tongue red and clean; bowels twice freely opened; has vomited several times since yesterday; wound granulating and looking well. To continue the same plan.

27th, Sunday, nine A.M.—Had suffered from occasional deafness and *tinnitus aurium* yesterday. This morning she is not so well. Pulse 125 and tremulous; the *tinnitus aurium* and deafness still continue; has numbness of one arm and leg; the bowels not moved. To take a little milk, to have a warm-water enema administered, and afterwards one of milk.

To take sesquicarbonate of ammonia if required.

28th, Monday, half-past nine.—Yesterday not so well; had delirious rambling. Pulse from 120 to 130; great restlessness and tossing about; was low in spirits; seemed much weaker; the bowels were moved; has taken the ammonia. She is much better this morning, and has had some sleep. The wound was patulous, and from it a dark-coloured and fetid fluid escaped. To continue milk diet; warm water to be thrown *per vaginam* into the uterus.

29th, Tuesday, half-past nine A.M.—All the symptoms are better; the wound is filling up by granulation; one ligature came away. The bandage was wet from the water, which had been injected *per vaginam* into the uterus, escaping through the wound. Slough over the sacrum came away, and the sores are looking well. Collodion to be again applied; to have a warm-water enema first, and afterwards one containing ox-gall if required.

From the above date, up to June 7th, nothing occurred in the character of the symptoms to require particular comment. She continued progressively to improve. The wound gradually filled up by granulation, and it is at this time nearly healed. The fistulous opening, through which the water which had been injected *per vaginam* into the uterus had escaped, is now completely obliterated. The sores over the back part of the sacrum, and on the nates, are also quite well.

During this period the diet chiefly consisted of milk, but towards the end of it animal food was allowed once a day. The mucilaginous mixture, with *Sodæ Sub Boras*, was the only medicine which she took, except the gum-water. When the bowels required relief, an enema of warm water was first administered, and, if necessary, this was followed by one containing ox-gall. The collodion was continued as a dressing to the raw surfaces behind, until the latter part of the time, when pads of cotton, with a mild unguent, were substituted. During my absence from Manchester, I received favourable reports of the patient from Mr Gibbon, under whose professional care she was placed, in consequence of Mr Cluley's severe illness. In his last letter, dated June 18th, he says:—"The wound is very healthy, but not quite healed."

June 26th, Tuesday.—I visited her along with Mr Cluley, and found her down stairs and looking very well; she remarked she was in excellent health. On removing the dressings we found two or three spots of exuberant granulations, which only required the application of nitrate of silver and a little dry lint.

July 15th, Sunday.—I called on Mrs Haigh. She was looking extremely well and in excellent spirits. She observed that she was better, and could walk with more ease to herself than she could have done for a long time before the operation; the wound was quite healed.

It was a great object with us that the infant's life should be preserved; we therefore strenuously recommended a wet nurse, and, if one could not be obtained, then that it should be supplied with ass's milk; but, from unavoidable circumstances, neither were procured until its life was placed in great danger. All those mischiefs consequent upon dry nursing appeared; such as bowel affections, a threatening of marasmus, and convulsions. At length a nurse was obtained, after which the infant improved, and on this day is quite well.

Before I proceed further, I take this opportunity of mentioning that the surgical part of the operation was most skilfully and dexterously performed by Mr Cluley; and his punctual, assiduous, and unremitting attention to the patient, are highly honourable to him. To him I am personally indebted, and return him my sincere thanks for his uniform great kindness and courtesy.

Remarks.—Mary Haigh was occupied before her marriage as a domestic servant, and was then strong and capable of undergoing great exertion. She is of a sanguineo-lymphatic temperament; her skin fair, with a red blush on the cheeks; her hair of an auburn or reddish-brown colour; the tint of her eyes is rather peculiar, being of a brownish-grey, and they have an animated expression. Her father is now living, and very healthy. Her mother has been dead many years, and most likely her death was occasioned by some chronic disease of the vertebræ, as I understood she was afflicted with abscess in the back.

Our patient is thirty-one years of age, and has been married nearly nine. During this period she has had five children. The labours of the first four were natural and quick; the last of this number happened three years ago, and was so rapid that the infant was born before the obstetrician arrived. After the birth of the second, she was rather more delicate, and suffered a little from indigestion; and about five or six years since first complained of slight rheumatic pains about her hips. Two years since she was confined to bed for a short time by pains about the pelvis; but she gradually recovered, and afterwards was able to walk about tolerably well. Her general health remained the same up to the period of her last pregnancy. She was now observed to limp a little when she walked, and to be less in height.

During her gestation her progression was more difficult, and her gait more waddling. She also complained more of pelvic pains; and the diminution in her stature now evidently increased. *Mollities ossium*, the disease under which she suffered, usually commences during pregnancy, and generally becomes suspended in the interval, returning in an aggravated form in each successive pregnancy, until its ravages have completely destroyed the form of the pelvis. In this case, however, it did not exactly pursue this course. There is no doubt there existed a strong predisposition to the disease—most likely hereditary; and probably the disease began at the latter part of the second pregnancy. But evidently no great, if any mischief, was done to the pelvis at this time, or for a long time after this period, as the third and fourth labours were so rapidly and easily terminated. The rapidity of its progress is remarkable; for there is little doubt that the great degree of distortion took place immediately before and during the last pregnancy. Sometimes in this disease, the bones are so soft that they yield, when the hand is introduced to make an exami-

nation. This happened here, as Mr Cluley thought he felt a giving way of the bones when he examined the pelvis.

Sometimes the pelvic bones, when affected with this disease, yield during labour, when the infant is drawn through. Three cases of this kind have occurred to my knowledge, one of which is detailed in the Provincial Medical and Surgical Journal, 1847, p. 706.

Opium is usually given after great operations to lessen the shock on the nervous system; but in the present instance we had no evidence that such an effect existed, and therefore on this account the drug might have been omitted. A second dose was administered by Mr Cluley, to which he attributed the vomiting which afterwards occurred. He considered that it had produced an effect similar to that which follows a debauch. It most likely constipated the bowels, but there is no doubt that this was chiefly caused by the bowel being compressed between the bulky uterus and the projection of the lower lumbar vertebræ and promontory of the sacrum. The garrulous delirium, the convulsive twitchings, and *tinnitus aurium*, &c., were considered by Mr Cluley to depend on a state requiring more support; we therefore agreed to give a milk diet, and, as its effects were so satisfactory, it was continued to the end.

The negative system of treatment here pursued considerably contributed to the well-doing, both of this case and also of the one in which I was concerned along with Mr Goodman. I have also observed the same plan, most beneficially carried out, in the after-treatment of abdominal sections for the extirpation of large ovarian tumours. There are great objections to the use of purgatives after these great operations, as the mucous membrane of the bowels is so readily disturbed; we therefore only ordered two doses of pills, and trusted chiefly to the use of enemata. The ox-gall enema was decidedly beneficial.

Before the incision was commenced a question was put to me by one of the gentlemen present, whether it was considered necessary to administer chloroform? I answered in the negative. I objected to its use on two grounds;—1*st*, Because it was unnecessary; the patient possessed in such a high degree tranquillity, calmness, and resignation of mind. Moral courage is superior to anæsthesia. 2*d*, Our experience of it in this operation is limited, there being at that time only one case published, in which this plan had been adopted, and that an unsuccessful one.—*Vide* Lancet, vol. i., p. 139, Feb. 1847.*

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* In addition to the above objections to the use of chloroform in cases of Cæsarean section, I quote one of the conclusions drawn by Dr Denham, which I have met with since my paper was read before the members of the Provincial Medical and Surgical Association, in his "Report upon the Use of Chloroform in Fifty-

Since then, another fatal case of Cæsarean operation, in which chloroform was used, has been detailed by Mr John Campbell.—*Vide* London Medical Gazette, vol. xliii., p. 1105, June 22d, 1849.

Rupture of the membranes, and evacuation of the *liquor amnii*, a long time before the operation, is always to be deplored. But although this accident had happened here, yet the great bulk of this fluid was still retained in the middle and upper portion of the uterus, which felt fluctuant and round in shape, and which admirably prevented the contraction of this organ, and so thereby lessened the chance of mischievous pressure on the maternal structures, and also contributed to the safety of the infant, and rendered its extraction more easy. The length of the uterine wound was also thereby diminished, in a degree proportional to the difference in the measurement of the uterine tissue, when distended by the contained fluid, and after its evacuation, when shortened by contraction.

The dangerous results of protracted labour are to be found in all the published cases of this operation, which have had a fatal termination, and which ought never to be allowed to occur in any cases of labour in which such a degree of deformity exists as to require its performance. In the present case, happily, the water was not evacuated until a short time before the operation, and then only very partially; the pains were also fortunately so slight, that no injurious pressure was made.

Fæcal accumulation generally takes place during ordinary pregnancy, if great attention be not paid to prevent it; but in cases of distortion of the pelvis this is much more likely to happen, from the mechanical impediment offered by the unnatural projection of the promontory of the sacrum, and lower lumbar vertebræ, and also from the anterior, oblique, and flexed position of the body and *fundus uteri*, which throws the cervical and oral portions of this organ backwards. The interposed rectum is thereby compressed, and the passage of the fæces interrupted.

The evils of a neglected state of the bowels are not felt so much during pregnancy as they are afterwards, in the puerperal state, and which in some cases lead to a fatal result. It must be quite obvious then, when we contemplate an operation, such as the Cæ-

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six Cases of Labour." He says,—"If given in large quantities, or if persevered in too long, it puts a stop to all muscular action. This is contrary to the opinion of Dubois, who states that it never destroys the uterine contractions, or those of the abdominal muscles."—Vide Dublin Journal of Medical Science, No. xv., p. 107, August 1849. Notwithstanding the high authority of Dubois on this subject, Dr Denham's numerous facts are sufficient to induce us to believe in his opinion; and if it be the case that chloroform does interfere with regular uterine contraction, we ought not to have recourse to it, or any other means which act in this manner, in cases of Cæsarean section.

sarean section, that the bowels should be first unloaded; and if this has not been fully and naturally accomplished, an enema, consisting of a large bulk of warm water, should be administered. In the present case, the free evacuation by stool contributed to the safety of the patient.

The aid derived from auscultation, to determine whether the child be living or dead, is most valuable, but not more so than the evidence it furnishes us as to the location of the placenta. It is of the utmost importance to avoid, if possible, cutting into this organ; for if it should happen that it is fixed on the portion of the uterus incised, there is some risk of hæmorrhage, and it may lead to irregular contraction of the uterus, and so become an obstacle to the speedy and safe extraction of the infant.—Vide case by Dr Henderson, Edinburgh Medical and Surgical Journal, Vol. xvii.; also case by me, ibid. Vol. lv., p. 67, case 2d.

The same accident happened again in my unpublished case. Happily in the present case we determined on the site for the incision, which was free from placental attachment; and it is remarkable how regularly the uterus contracted. The analytical tables of all the Cæsarean operations performed in Great Britain and Ireland, which were laid before the profession in 1843, with those added which have since occurred, furnish important data on several practical points. They would, however, occupy too much space to introduce here. One inference to be drawn from them shall be mentioned. It is the black account of the great maternal mortality shown to arise from this operation. Out of fifty women operated upon, five only have been saved. At the present time it is not my intention to inquire into the validity of these five cases. The statistics of the results of this operation thus appear very unfavourable, and if abstractly considered, are quite enough to deter us from its performance. But before we condemn this expedient, we ought carefully to analyze each case; to ascertain what proportion of the fatal issue is really attributable to it; what part is assignable to the constitutional or local state of the patient; what is due to the length or the influence of the labour itself, or its management; and at what period, and how, it was performed. All these subjects have received my serious attention, and, as far as possible, each individual point has been faithfully recorded by me, and the deductions truthfully stated to the profession. I will now place before you the result of five cases which have occurred to me:—

Of the five women operated upon,	2 saved;	3 lost.
Of the five infants extracted,	2 ...	3 ...

One of the women who died had been in labour thirty-four hours; the membranes were ruptured two hours afterwards; the pulse was 150 in the minute, and feeble; repeated vomiting had occurred;

the patient had great tenderness in the belly, which was considerably increased by pressure; thirst was great; tongue furred and dry; great anguish was expressed in countenance; and the external genitals were much swelled, and the vagina hot, dry, and rough. On withdrawing the hand an odour was perceived from it similar to that which takes place from partially decomposed animal matter. The movement of the infant had not been felt for some time, and its heart could not be heard by the aid of the stethoscope. When extracted it was dead.

In another of the cases which occurred, the woman had been in labour and the membranes had been ruptured twenty-two hours; the pulse was 120; skin hot; tongue furred; thirst was urgent; pains very frequent; and the patient had in the belly great tenderness, which was considerably increased by pressure. The infant was alive, but was destroyed by being spasmodically seized around the neck by the uterus.—Vide Edinburgh Medical and Surgical Journal, Vol. lv., p. 67.

In the third case the duration of labour was fifty-three hours, and the membranes had been ruptured fifty hours; abdomen excessively tender; fetid discharge from vagina; pulse 130, irritable and weak; bowels had not been moved for several days; vomiting was frequent; skin hot; thirst great. The incision being made, the intestines were exposed and much inflamed; there was some serous effusion of a red colour in the belly; peritoneal coat of the uterus injected. The infant had not been felt by the woman to move, nor could the pulsation of the heart be perceived by the stethoscope. When drawn out it was putrid.

From the above statement of the condition before the operation, of the three women who died, we are warranted in concluding that their deaths were not attributable to the operation. We have in all of them indisputable evidence that the mischief was occasioned by protracting it, more especially in the two last-mentioned cases.

Of the three infants extracted dead,—in that of the first case we have every reason to believe it was so before the operation, as motion was not felt by the mother, nor could we hear the pulsations of the heart. In the second case, the infant was alive, and its death is no doubt chargeable to the operation; but it was produced by a cause which I think may in general be avoided, or at least guarded against. The third to be accounted for was already dead and putrid.

Notwithstanding the unfavourable aggregate results of the Cæsarean section in Great Britain and Ireland, I think, from my own experience, shown in the above statements, I am justified in advocating it as an operation of election, not merely having recourse to it as one of necessity, according to our present obstetric rule, when no other means can suffice, but to give it a preference over the use of the crotchet, in cases when neither premature labour, the long forceps, these two operations combined, or turning, will meet the exigencies of the case. I am a warm, and I hope a sincere, advocate for its adoption, and doubt not but if it is early and properly performed, and if the after treatment is judicious, it will be even more successful than it has been in my hands.

The maternal mortality will thus be considerably reduced, and will bear a comparison with that of other capital operations, provided a just calculation be made of all contingent and relative circumstances existing in each case. The number of deaths will no doubt then fall considerably under that of those following the use of the perforator and crotchet, although it is now stated to be the reverse, but the truth of which statement I have great reason to doubt. The results to the mother, in all cases of the Cæsarean section, are known; but those after the use of the crotchet are buried in oblivion, the grave telling no tales. The child's rights are maintained by the former; for if living it may be extracted alive, but by the latter it is necessarily destroyed.

ART. V.—*Upon the Structure of the Kidneys.* By Dr GERLACH of Mainz. (Muller's Archiv. fur Anatomie, Physiologie, und Wissenschaftliche Medecin. 1848. Seite 102.)

[IN the sixty-eighth volume of this Journal, October 1847, was published an account of the minute anatomy of the kidneys, as revealed by microscopic examination, from the researches of John Muller, Mr Bowman, Professor Reichert of Dorpat, Dr Gerlach of Mainz, F. Bidder of Dorpat, A. Kolliker of Zurich, and Mr Toynbee. From the accounts given by these different anatomists, it results, that some difference of opinion was entertained regarding the mode of connection between the capsule of the *Corpora globosa* of Malpighi and the blood-vessels on the one hand, and the uriniferous tubules on the other. The statements of each anatomist, and of the opposing parties, we gave as exactly and faithfully as possible, in the manner and the terms in which they were given by the authors; and we gave no opinion on the merits of the question. The point is still under discussion. Dr Gerlach adheres to his original opinion; and, in the following paper, adduces those facts and arguments which he conceives are adequate to establish the truth of his views. His essay is instructive, as giving a short view of the controverted points, and shewing how the objections urged against his views may be explained and obviated, as well as in furnishing facts which may enable readers to judge for themselves.

In reading the present memoir, therefore, it will be necessary to consult, if not to peruse attentively, the article in the sixty-seventh volume; and, after giving this general recommendation, we think it unnecessary to make particular references to the opinions and hypotheses advanced by each of the authors.

It may be observed that this question,—being one not only of microscopical observation, but of the interpretation of microscopical appearances,—bids fair to be a considerable time a source of discussion and controversy among anatomical and physiological authorities.

It is remarkable that Gerlach makes no mention of the descriptions given by Mr Toynbee.]

Since the appearance of my first essay on the structure of the kidneys, in the fourth number of Muller's Archiv, for the year 1845, there have followed not fewer than six treatises upon the same subject; a proof of the great importance of several of the points brought under consideration, not only in an anatomical sense, but also in that of general physiological relations. This circumstance may form an excuse for me once more reverting to the subject of the structure of the kidneys; and, indeed, so much the more, that upon one principal question, namely, whether the communication alleged by Mr Bowman to exist between the urinary *canaliculi* and the capsules of Muller exist or not, opinions are more divided than ever.

Kolliker declared himself, in the subsequent number of these Archives, in favour of the communication, and indeed altogether in the manner in which this has been described by Bowman. Further, Bidder, though in his first treatise, with reservation that he admits an introversion of the membrane, forming Muller's capsule round the Malpighian vascular bodies, yet in his second essay, to be immediately mentioned, he again gives up this exception. In like manner, Mandl is in favour of the communication. This anatomist admits a peculiar secretory canal in the capsule, and undertakes to distinguish this from the urinary tubules. But, with every one who has accurately studied the structure of the kidneys, the statements of Mandl admit not this interpretation; since in a subsequent passage he considers the empty vessels of the Malpighian bodies to be glandular ducts, and says that injection undertaken in order to determine whether these canals belong to the vascular bodies, or are peculiar gland-tubes, have as yet furnished no positive result, while only the simplest injection from the renal arteries is sufficient to afford conviction, that it is impossible to speak of glandular *canaliculi* within the capsule. The cells first described by me as enclosing the Malpighian vascular bodies, he admits as lying in the interior of these alleged glandular canals, and thinks that he establishes in this manner a complete analogy between the Malpighian vascular bodies and the other glands, since he believes that he has proved in these the three essential elements of glands; namely, glandular canals, cells lying within these canals, and excretory ducts.[*] In Paris, where observers work in general little with the microscope, so surprising an analogy may for some time excite a certain sensation; but, to German anatomists, it is much too exact.

Patruban also fully agrees with the views of Bowman, from researches made on the *Coluber Natrix*, and the human body.[†]

Against all communication Hyrtl declared himself, both on the evidence of multiplied injections, and also on that of the results obtained by tying the ureter, after which a manifest dilatation of the urinary tubules, yet no change in the capsules, is wont to take place. This declaration of Hyrtl possesses so much more weight, that this anatomist is certainly with justice accounted the greatest master in the technical details of injection, and because he further states, that he has the charge of a collection of near one hundred microscopical preparations, each of which furnishes a completely conclusive proof against the correctness of the new doctrine.[‡]

From observations on the naked AMPHIBIA, especially the Tritons, Bidder has in his most recent memoir[§] arrived at the conclusion, that there are undoubtedly dilatations in the urinary tubules, which he regards as identical with the capsules, but that the Malpighian bodies do not lie within these expanded portions, but beneath them, and are connected with them only by filamento-ligamentous tissue. Bidder accordingly assigns to the question of communication a position quite peculiar, since he agrees with the fact of the investing membrane of the Malpighian vascular bodies being enclosed in a capsule, which was discovered several years ago by John Muller in the naked AMPHIBIA.

In the following observations I propose to adduce the proofs in favour of my own opinion, that there actually is a connection between the urinary tubules and the capsules, and I shall comprise what I have to say under four heads.

1. By injection upwards from the ureters, I have succeeded in several instances in filling the capsules with injection matter, particularly three times in the sheep, twice in the frog, and once in the horse. I have experimentally injected upwards from the ureter more than three hundred kidneys of different animals; and

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[*] Louis Mandl, Memoire sur la structure interne des Organes Urinaires. Archives d'Anatomie Generale, p. 284: und Anatomie Microscopique, 16me livraison Organes Urinaires.

[†] Beitrage zur Anatomie der Menschlichen Niere, von Prof. v. Patruban, 15ter Bd. der Prager Vierteljahrsschrift, p. 87.

[‡] Beitrage zur Physiologie der Harnseckretion, von Prof. Hyrtl. Im zweiten Bande der Zeitschrift der Gesellschaft der Aerzte zu Wien. Seite 381.

[§] Vergleichend Anatomische und histologische untersuchungen uber die Mannlichen Geschlechts und Harnwerkzeuge der Nackten Amphibien. Von Dr F. H. Bidder, Prof. der Physiologie in Dorpat.

among these numerous trials only six times have I obtained satisfactory results,—a circumstance which is certainly the strongest proof of the difficulty of this undertaking. The reason why Hyrtl never succeeded with this mode of injection, I believe we have to seek in the composition of the injection matter employed, which, as he states in the course of his treatise, contained oily and resinous substances. In my judgment, only gelatinous substances are capable of leading to proper results. Bowman himself, who showed me when I was in London all his injected preparations, has not one to show the capsules injected from the ureter; all his injections were made from the arteries. The preparations of this anatomist, nevertheless, particularly those of the serpent's kidneys, are very conclusive as to the reality of this communication. Professor Schroeder van der Kolk, however, showed me in his museum, which is in many respects instructive, the kidneys of Crocodiles which were injected from the ureter. In these animals the urinary tubules, as well as the capsules, are unusually large, and accordingly the injected capsules may be easily distinguished by the naked eye.

As an authority on this subject so great as Hyrtl declares the injection of the capsules from the ureter impossible, I think that it is my duty to adduce in regard to my preparations, the following fact. Injected capsules of the kidneys of the sheep, as I delineated them in my first essay, have been witnessed not only by my numerous friends in Paris, but by the present Professor C. Vogt and Dr Pappenheim, who have thereby convinced themselves of the accuracy of my views. My honoured friend Vogt has, upon the suggestion of Milne-Edwards, made before the Philomathic Society, upon the point under question, a report, and which I am not yet so practised in the French language as to read before so distinguished an assemblage.

The injection material employed by me possesses this advantage, that preparations made by it look much better when they are examined by transmitted light, by which also the objection urged by Hyrtl is obviated, namely, that occasionally lateral extravasation of the material has been mistaken for the injected capsules. In my preparations it is possible to follow the course of the structureless membrane of the urine-tubules, since these were not dried, but were preserved between two glass plates in water, to which some spirit of wine was added. In the course of last summer, I fully succeeded in injecting the capsules of a frog's kidney from the ureter; and I accordingly hastened with my preparations to Heidelberg, to show them to Professor Henle, who was on this question a competent judge. This anatomist was convinced in the most unequivocal manner of the communication, and expressed his regret that he had already sent to press his report on the

Advances in General Anatomy for the year 1846, in which, following Hyrtl and Bidder, he had declared himself against the connection of the capsules with the urinary tubules.

As to the form of the communication, I have, in my former memoir, declared myself for the lateral insertion of the capsules in the urinary tubules. In the kidney of the sheep and the horse, this representation is perfectly correct, as I have subsequently repeatedly satisfied myself. In the horse, nevertheless, the *cervix*, or contracted portion of the capsule is more lengthened than in the sheep. In the frog, on the other hand, the communication of the capsule with the urinary tubules is rather a terminal point; this, nevertheless, is to be understood not absolutely; but we may express the fact in this manner, that the *cervix* or neck, by which is distinctly understood nothing but the small contracted portion of the urinary tubule, is longer than in the horse, and especially than in the sheep. By the neck or *cervix*, then, we understand the part of the urinary tubule which lies between the ultimate dichotomous division and the capsule.

2. To every one who is conversant with the details of injection, it is known with what facility, in injection by the arteries, the urinary tubules are filled with injection material. This circumstance has led many anatomists, as also recently Berres, to maintain a direct connection between the blood-vessels and the urinary tubules. This occurrence may be explained simply by the direct communication between the capsules and the urinary tubules; because to the injection-material, if it is extravasated by laceration of any point of the Malpighian vascular bodies, no other path remains than that into the urinary tubules. If we admit no communication, it is impossible to perceive how the injection-matter can penetrate into the urinary tubules; it can at most lacerate only the capsule without the wall of the Malpighian body, and then be effused between the urinary tubules. But further, the attentive examination of those preparations, which being injected from the arteries, show the urinary tubules to be filled, proves that the arterial vessels have no other connection with the urinary tubules, than by the medium of the Malpighian vascular bodies. Here only is to be found the point of extravasation, the proximate cause of which consists in the conditional limitation of the proportional progress of the injection-material through the multiplied convolutions of the vessels. In consequence of this facility of extravasation in the Malpighian vascular bodies, injection from the arteries is the most convenient method to procure injected urinary tubules; and almost all the preparations in the valuable collection of Mr Bowman, who from his position can command the possession of the kidneys of most animals, have been made in this manner.

3. The facts of Comparative Anatomy speak in favour of this

communication, as John Muller, in the conclusion of his Memoir on the Comparative Anatomy of the Myxinoid Fishes, has distinctly shown. I must confess, that I cannot rightly understand what is the use of Comparative Anatomy, if it do not furnish us, by comparison of the simple structure of the organs of the lower animals, conclusions on the complicated structure of those in the higher classes. Now exactly in this respect are the lowest animals of the Vertebrated Class, namely, the Myxinoid Fishes, of the highest interest, and in these the kidneys are certainly formed on the most simple type. And wherefore should the inquirer dispute in the higher animals an arrangement which is so distinctly presented in the lower orders, only because in the higher animals the relations are complicated, and are more or less withdrawn from investigation.

4. The communication between the capsule and the urinary tubules involves no physiological impossibility, as Bidder, and especially Reichert, maintain. The anatomist may view the glands as simple filtering apparatus, or search for the essence of secretion in the metabolic force of the gland-cells; yet the essential elements of the glands are, in all cases, the gland-cells, which, according to the one view, are the agents of filtration,—according to the other, of conversion.

The existence of cells, which are continued from the wall of the capsule upon the Malpighian vascular bodies, and enclose them, has been already proved in my first essay, and since that time the fact has been confirmed by Kolliker and Hyrtl. Bidder, who denies the existence of cells, reproaches, indeed, Kolliker and myself, that we had mistaken for gland-cells blood-corpuscles which had been changed by water. But this insinuation I must decidedly repel. More than six years' daily practice with the microscope must, I trust, give sufficient assurance, that I am capable of distinguishing between gland-cells and blood-corpuscles. Besides this, we observe regularly in the Malpighian vascular bodies rather *nuclei* than proper finished cells; and, with the exception of the Tritons, I know no animal which has blood-corpuscles of such large dimensions as to render it possible to confound them with the nuclei of the gland-cells. These cells sink in the different deep hollows which the Malpighian vascular body presents in its convolutions, within and around these hollows so completely, that they may be said to lie naked in the interior of the capsule. Accordingly, this is throughout no direct connection of a gland-canal with a blood-vessel, but between the wall of the blood-vessel and the interior of the capsule lie the most essential element of a gland, namely, the cells.

With the same reason must Bidder and Reichert pronounce to be a physiological impossibility the view which Henle has brought forward on the structure of the liver, in his System of General Anatomy. On this topic Henle expresses himself in the following manner:—"Anatomists represent to their minds the parenchyma of the liver as a compact mass of cells traversed by blood-vessels, which, yielding to each other, leave free cylindrical cavities, in which the excreted substance is collected." Here it is distinctly stated that the gland-cells are not separated by any peculiar gland-membrane from the wall of the blood-vessels; and, nevertheless, it has not yet occurred to any person, so far as is known to me, to find in this statement any physiological impossibility, or any contradiction to the known laws of the organization.

I think, further, that this may be the proper place to show the incorrectness of the view taken by Bidder, according to whom the Malpighian vascular bodies ought to be situate not within the capsule, but without it, and to be connected with it only by filamento-ligamentous tissue. Clearly to infer from this statement that, according to this intuitive fashion of reasoning, it is impossible to perceive any reason why in all cases a vascular body is attached by ligamentous tissue to a capsule, and not every other convoluted urinary tubule; this is contradicted by the results of injection. If, particularly, the anatomist has the good luck to fill the capsules by injection from the ureters, these are notwithstanding not so completely filled that there is not left at least one-third part of the capsule untouched by the injection material; and, upon careful examination, it is found that this third part includes the vascular body compressed by the mass. Further, notwithstanding much labour expended in this research, I could not find in the kidneys of the male Tritons those anatomical circumstances as Bidder has described them. Assuredly seminal tubules proceed from the *testes* to the kidneys; but I could never trace any connection between these seminal canals and the urinary canals, in the manner in which Bidder has described this connection. Never did I observe any enlargement or dilatation (capsule), which was in connection on the one side with the seminiferous tubules, and on the other extremity with the urinary tubules; and, nevertheless, I had, in strict adherence to the direction of Bidder, cut out *testes* and kidneys in the most careful manner, and introduced a very small glass plate between the two *testes.* Doubtless the upper portion of the Triton's kidney is remarkably well adapted for investigations on the relation between the capsule and the urinary tubules, since this organ requires almost no further preparation, in order to show distinctly this communication; but I never could distinguish more than one tubule which was in connection with the capsule. The direct connection between the urinary tubules and the seminiferous ones is further rendered improbable, in my judgment, by the circumstance that the latter are thicker than the urinary tubules, and that the membrane forming them consists of ligamentous tissue,—a fact of which any person may be easily convinced by the effects of acetic acid. An uninterrupted connection between one membrane consisting of ligamentous tissue, and another which is void of structure, would be, under all circumstances, something out of the usual course.

Lastly, I have yet to answer some objections stated by Hyrtl, derived from the large size and the disproportion of the number of the Malpighian bodies to the uriniferous tubules.[1] The foundation of this objection, that the vascular bodies are in proportion neither to the magnitude of the animal, nor to the size of the kidneys, is found, on close examination, to be simply this, that in animals possessed of large blood-corpuscles, the calibre of the capillary vessels requires to be larger than in animals with smaller blood-corpuscles. But since to the constitution of any Malpighian vascular body a certain number of convolutions is required, so that organ must occupy so much more space, the greater the extent of the capillary vessels contributing to its formation; or, in other words, the magnitude of the blood-corpuscles is in direct proportion to the size of the Malpighian bodies. Further, it is equally manifest that a kidney, the vascular bodies of which are very large, cannot contain so much as another kidney which has very small vascular bodies, since otherwise the space for the convoluted uriniferous tubules would be too small. There is, consequently, for the number of the Malpighian vascular bodies, the following law:—The size of the blood-corpuscles is with the number of Malpighian vascular bodies in inverse proportion. This is naturally not to be understood of whole or entire kidneys; but the law holds good for large portions of different animal kidneys. To these two simple formulæ may be referred the at first sight inexplicable relations between the size and the number of the vascular bodies in the different animal kidneys.

The interesting discovery by Hyrtl, in the difference of area in the ingredient and egredient vessels, I can only confirm; this circumstance is particularly obvious in the AMPHIBIA with large vascular bodies, and in the *Triton tæniatus* the calibre of the ingredient vessels amounts to double that of the egredient vessels.

Finally, in regard to the ciliated epithelium in the kidneys of the amphibious animals, my researches directed to that subject have taught me the following conclusion. It is in all instances merely a fortunate accident, when we find ciliated epithelium in a frog's kidney; often have I in the kidneys of two or three frogs sought for it in vain; the observer finds this equally in summer-frogs as in winter-frogs, in male as in female individuals.

Uniformly, as in the frog, is the ciliated epithelium in the triton; nevertheless, we do not in all cases find it. The rule is that we observe only a third part of the inner wall of the capsule ciliated. Sometimes, however, I have also observed the ciliated motion in the largest portion of the capsule, as far as the spot of entrance and the exit of the vessels; meanwhile the ciliated motion is particularly vigorous in the neighbourhood of the entrance-spot, and at the commencement of the urinary tubules. The reason to which I ascribe the circumstance, that we cannot demonstrate the ciliated movement in every amphibious kidney, I shall more fully determine in my work on General and Special Histology, which is forthwith to appear at Leroux's Court Library at Mainz.*

ART. VI.—*Documents on the Hypothesis which ascribes Cholera to the presence of Fungi.*

IN the seventy-second volume of this Journal, July 1849 (p. 208), it was shortly mentioned that Dr Cowdell had advanced, in order to explain the origin and nature of the symptoms of cholera, the speculation, that this disease is owing, or might be owing, to the presence and growth of *fungi*, or parasitical plants, in the human body. A short summary of the principal points in this hypothesis was at the same time given; and it was added, that "the chief reason for giving this brief mention" of the hypothesis at that time, was, "that it may be made known, and that it may be subjected to the test of observation. Against the hypothesis," it was said, "various objections might be stated; but in its favour are also other circumstances; and it is only by observation that its accordance with facts can be ascertained."

This hypothesis has, since the time now referred to, acquired first considerable importance, and subsequently been reduced within narrow and probably just limits. Observations made by Dr Brittan and Mr Swayne of Bristol, and Dr Williams of Swansea, showed that in the discharges of cholera patients were contained many minute bodies which resemble fungous productions, and seemed thus to confirm the hypothesis of Dr Cowdell. Subsequently, however, it was ascertained by Dr Baly, Dr Gull, Dr Jenner, and Mr Marshall, that these alleged *fungi* were in some instances remains of articles of food, in other instances actual *fungi* of substances taken into the stomach, either with articles of food, or with water used as drink; but that they were neither peculiar to cholera, nor had any necessary connection with the presence of that disease. The inquiry is one of great importance, as it serves to show the sources of fallacy by which microcopical observation is beset, as well as the means it possesses, when adequately and properly conducted, of correcting these errors. For these reasons, it seems proper to present in succession the most important papers which have been recently published on this subject, and especially the instructive Report from the Sub-Committee of the Royal College of Physicians.

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* This work has, since the appearance of the above paper, been published; the first part in 1848, and the second in 1849.

Justice also seems to require that the doctrine, as originally proposed by Dr Cowdell, be more fully stated.

Dr Cowdell undertakes to prove the justice of his general proposition by establishing five inferences, as preliminary and fundamental.

First, he undertakes to prove the aptitude of *fungi* for growing or being developed in organised bodies,—animals and plants.

Secondly, he adduces, as illustrative arguments, some of the most remarkable among the known effects produced by organised beings of the order PROTOPHYTA; (*Fungi; Algæ*).

Thirdly, he shows the capability of *fungi* to produce the phenomena of pestilential disease, as exhibited in cholera.

Fourthly, he considers the circumstances which probably concur in producing from the germs of *fungi* the effects of pestilential disease, and adduces various facts to prove that a variety of *nidus* causes a variety of *fungus*.

Fifthly, he shows that the prevention or destruction of fungous germination, and the consequent catalytic actions, is effected by the same medicinal agents, which are recommended, on the highest authority, in the prophylaxis and early curative treatment of pestilential cholera.

In establishing the first inference, Dr Cowdell refers to the instances, now multiplied, in which fungous productions have been found in the bodies of animals and vegetables. Of this kind are the muscardine of the silk-worm, the sarcina described by Mr Goodsir in this Journal, and a parasite found in thrush; to which may be added, the instances of *microsporum* discovered by Gruby and Malmsten in the *porrigo decalvans.*

These facts, he contends, show the possibility of *fungi* growing in the animal body.

In proving the second inference, Dr Cowdell adduces various facts, from which he draws two conclusions.

Dr Cowdell infers first, from a general view of the occurrence of fungous productions in the bodies of various animals and in such processes as fermentation, that the infinitely minute germs or sporules of *fungi* are at all times present in the air breathed by animals, and, as it were, seeking an opportunity, in the very first tendencies to decomposition of organised matter, vegetable or animal, dead or alive, to germinate and reproduce themselves, though in form ever varying with peculiarities of *nidus* and aliment; and that these *fungi* do by their germination effect changes and transformations only recently discovered to be ascribable to such agency;—namely, dry-rot, smut, rust, ergot, mildew, mould.

A second conclusion deduced by Dr Cowdell is, that processes hitherto regarded as purely chemical and inorganic, namely, the different kinds of fermentation, may with much probability be assigned to the province within which the phenomena of fungous germination take place; and, indeed, that in one of these processes the germination and reproduction of *fungi* has been demonstrated as the acting cause. This view so well harmonises with what is known of the modes by which the *Protophyta* (*fungi*, &c.) propagate themselves; and the circumstances, preventive and promotive, in both cases so nearly correspond, that the inference of *Identity in Generic Properties* he considers nearly to approach to demonstration.

In order to prove the third inference, Dr Cowdell compares the effects of the use of the *fungi* either accidentally taken with food or otherwise introduced into the system; and by referring to the effects produced by the use of spurred rye and poisonous sausages, he maintains that the phenomena following the use of these articles in the human body are quite similar to those presented by pestilential cholera. The general inferences under this head have been already given in the seventy-second volume, to which accordingly the reader is referred.

Under the fourth division of argument, or that relating to circumstances, which probably concur to produce from the germs of *fungi* the phenomena of pestilential disease, and facts showing that a variety of *nidus* produces a variety of *fungus*, Dr Cowdell arranges all those accessory circumstances which are found in peculiar states of the atmosphere, certain conditions of the telluric, geological, and physical characters of particular spots on the globe, electrical states of the earth and atmosphere, magnetic phenomena, and the operation of vegetable growth in generating electricity in the atmosphere. The sporules of *fungi*, he argues, may lie long, that is, for months and years, in a dormant and inactive state, until called into action by certain conditions of the surface of the earth and certain states of the atmosphere. Extensive Deltas, such as those of the Ganges and Indus, are favourable situations for the formation and the deposition of these sporules, which in certain seasons and during years may be dormant, and in others may be roused into energetic growth and development.

As *fungi* are meteoric, so may pestilence, and in particular pestilential cholera, be in like manner considered as meteoric. As the sporules of *fungi* possess a peculiar electrical condition, they may probably be attracted by the lungs of certain persons and repelled by those of others; persons and their several parts being shown to possess each its peculiar state of electricity, varying probably with the vicissitudes of electrical condition incessantly taking place in the atmosphere. Active vegetation is highly conducive to the development of those conditions on which meteorological disturbances depend; and vegetation of this description is abundant and constant in the climate of Jessore, as in many other places in India, in the Delta of which region are found all the elements adequate to compose a forcing bed for the growth of fungous vegetables. Similar are the physical and telluric properties of the region of Kurrachee in the Delta of the Indus, on the western boundary of Hindostan. Both these districts have been remarkable for the sudden and violent appearance of epidemic cholera; and in both instances the occurrence of the disease was preceded by remarkable meteorological phenomena. Cholera makes its attacks most commonly during the night; and during the same period the growth of *fungi* is most common and most active, probably in consequence of the presence of an altered state of electricity during the hours in which the solar light is withdrawn. *Fungi* are convertible; change of circumstance converting one variety into some other, so that the germs of one particular variety, under the operation of conditions highly favourable to their development in human blood, might in that *nidus* produce a distinct variety capable of reproducing itself in a similar *nidus* without the concurrence of those meteorological phenomena which co-existed at its first production. The germs of one sort of *fungus* might possibly, under varied circumstances, give rise to more than one kind of pestilence, since by several authorities several different forms of pestilence are considered as mere varieties of the same general type.

On the fifth division of argument it is unnecessary to reproduce the statements and reasonings of the author. They form in general that species of proof which is employed to demonstrate, from similarity of remedies, similarity of diseases.*

The summary now given presents a correct view of the facts and arguments employed by Dr Cowdell to establish the correctness of his hypothesis. It is unnecessary to consider their cogency and the degree of proof which they furnish; two observations may be submitted.

It is proper to observe that when the author states under the second head, chapter viii., that it is only recently that it has been discovered that such processes as dry-rot, smut, rust, ergot, mildew, mould, and similar changes depend on the growth of *fungi*, it is not easy to understand in what sense the term recently is employed. It is fully thirty years since the dry-rot in timber was observed to be co-existent with the growth of *fungi* in the timber. It is probably longer, certainly not shorter, since it was known to good observers that smut, rust, and mildew were in like manner connected with the growth of fungous vegetation. That the spur or ergot in rye and other grains was also connected with the presence of minute parasitical plants, has been known for a period still longer. And with regard to mould, we shall not say how long that production has been connected with *fungi*; but we are certain that nearer forty years ago than any other time, we have known that growth to consist in fungous productions.

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* A Disquisition on Pestilential Cholera; being an attempt to explain its Phenomena, Nature, Cause, Prevention, and Treatment, by Reference to an Extrinsic Fungous Origin. By Charles Cowdell, M.B., M.R.C.S. London, 1848; 8vo.

Whether these pieces of information have been of recent acquisition, readers will form their own judgment.

In the second place, it seems not very consistent with what is known of organic products, to say that *fungi* are convertible. If by this statement it be meant that one genus of *fungus* can be converted into another, we know of no facts which warrant such an inference. The idea approaches to that of equivocal generation. At the same time it is possible that this statement, which is made at page 190, may be owing to some obscurity or at least not very definite mode of expression.

It may be observed that Dr Cowdell shows only what may be the cause of cholera, that fungous productions may give rise to this disease, not exactly that they do give rise to the disease. That undertaking was reserved for the inquirers whose researches are to be immediately mentioned.

1.—*Report of a Series of Microscopical Investigations on the Pathology of Cholera.* By F. Brittan, M.D., M.R.S.C.L., &c., Lecturer on General Anatomy and Physiology at the Bristol Medical School.

The phenomena of such a disease as cholera,—a great pestilence, which, sweeping over the world, involves all lands and all nations in one common dread and mourning,—awaken an universal interest far more deep and anxious than any ordinary curiosity in the current of passing events. The medical profession has been appealed to, but with the effect, unfortunately, rather of increasing the mystery and apprehension, than of imparting confidence. It must be honestly confessed that we know nothing of the exact nature and cause of cholera; and in the absence of any one common point on which all professional men are agreed, it would seem as if each considered himself at liberty to throw out his opinions and theories, though based on no previously-recognised principle, and without a single well-ascertained fact to support them;—perhaps in the vain hope that at last, in their very multiplicity and variety, some one must hit upon the true explanation. Thus it is that our daily papers and medical periodicals teem with histories, theories, remedies, and even specifics, of so directly opposite character, all eagerly devoured by the public, and all alike falling before the test of inquiry and experience, until the disease has begun to be looked upon as a hopelessly inscrutable mystery, and anything that is put forward in regard to it as only another baseless speculation.

Influenced by these circumstances, I have been most anxious to avoid doing or saying anything that could add to the confusion, or mislead from the right path those who were really studying the disease; and I have refrained from making public my own opinions, until the facts on which they were founded had been submitted to the examination of those most competent to give an opinion on their validity. This I have now done. I have shown to some of the best microscopical authorities in the kingdom my own specimens prepared from cases described in Table I.,* and represented by engravings. I have substantiated them on examples furnished by these gentlemen themselves, and it is with their full concurrence and assurance of their importance that I lay the following facts before the profession. It must be borne in mind that they are put forward as facts, and not mere opinions, and that the validity of my statements have been and can be demonstrated to be true or false, by any one who will take the trouble. In order, however, to simplify the whole matter as much as possible, I shall confine myself to a plain historical detail of the investigations.

On Monday, July 9th, in conjunction with Mr J. G. Swayne, as fellow-member of a sub-committee appointed by the Bristol Medico-Chirurgical Society for the microscopic investigation of choleraic evacuations, I examined two specimens of rice-water dejection (numbers 1 and 2 in both tables); and on comparing our drawings made from them, and produced before the sub-committee, we were struck with the peculiar appearance of certain bodies depicted in each. On further prosecuting this investigation, I found these bodies to be constantly present in the rice-water evacuation of cholera patients, and offering the same characteristic appearance that distinguished them from anything I had before observed. In order to ascertain, whether they bore in their size or quantity any relation to the severity of the symptoms, I endeavoured to obtain specimens passed by the same patient at different periods, as well as to complete the observations by an account of his condition at the time. The result was, that as several cases in my table indicated, and as other cases not recorded seemed to prove, I became convinced that a certain relation does exist between the size and number of these bodies, and the time elapsed after the seizure, taken in connection with the severity of the symptoms. That is to say, they are small and clearly defined in the matter vomited; they become larger and more compound in the dejections; and as the disease advances favourably, where I have had the opportunity of examining, they vanish as the symptoms disappear, and the motions regain their natural appearance. I have also found that in very rapidly fatal cases these bodies are sometimes to be met with only in very small quantity, or are altogether absent; though this observation must be qualified by the remark, that it is not always possible, or at all events has not been with me, to obtain portions of every motion passed, and that these bodies might have been present in those not examined.* It must also be recollected, that but a small quantity is saved for investigation, and but an infinitesimal part of that even ever comes actually on the stage of the microscope. My observations contained in Table I. were made on cases taken just as they came under my notice, and without selection; some in the cholera hospital, some through the kindness of Mr Ralph Bernard, in the Bridewell. They extend in dates from July the 9th to July the 30th.

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* The Tables are omitted, as not likely to be read.

Whilst collecting this series, I examined and compared the specimens with others obtained from patients free from cholera. I found that in healthy solid motions these bodies did not exist, nor could I meet with them in the fluid stools of typhus and other diseases, but that they were present in the cases of severe choleraic diarrhœa so prevalent in districts where the disease abounds; and I was thus led to the necessary inference that these bodies were peculiar to the evacuations of cholera patients, and must have some essential relation to the disease.

The observations contained in Table II., collected by Mr Swayne, on cases in the cholera hospital, with the exception of Nos. 1 and 2 (the cases from the comparison of the drawings of which we were first led to notice these peculiar bodies), were made subsequently to mine, and date from August 2d to the present time. He was prevented from continuing these first investigations with me; and as his observations were taken quite independently of my own, which they so fully confirm, I have thought it better to give them in a separate form.

Having been thus led to consider these bodies, which, from the characteristic of their appearance, I have termed annular bodies, in some manner essentially connected with cholera, I wished to ascertain whether it might be as cause and agent, or effect and product. That it could not be the latter seemed evident at once from the fact, that they were unlike any of the known healthy or morbid elements of the body, or secretions; and as they were found in the vomited matters, apparently in an early stage of development, it seemed probable they were introduced from without, and would be met with in the atmosphere, &c., of places where cholera was rife. Accordingly, with the view to test the truth of this supposition, on July 19th, with the kind assistance of, and an apparatus suggested by, Dr Bernard, I condensed about one drachm of fluid from the atmosphere of a room in a house from which five patients had been removed the day previous to the cholera hospital, and found in it bodies of the same appearance as those represented in fig. 1. I soon afterwards repeated the experiment, with the aid of Mr Ralph Bernard, in a cell in the Bridewell, which had been unoccupied for some time, but adjoining cells the occupants of some of which had been seized with cholera, one of whom died the day before. Here, also, the same result was obtained; and from a specimen preserved, which has been carefully examined by many excellent microscopists, fig. 1 is accurately copied. The same experiment was then tried in situations free from cholera, but with a negative result: the fluid here obtained was destitute of these bodies, and contained only small portions of hyaline structureless matter, also observed in the first. I have since repeated these experiments, aided by Dr William Budd, several times with the same positive and negative results, and therefore feel justified in stating that the same will follow similar investigations made elsewhere, if the necessary care be taken, and a glass of sufficiently high power (I used a Ross's 1-12th) be employed. The only question remaining is, as to the identity of the annular bodies thus shown to exist in the atmosphere of cholera districts, and in the vomited matters and evacuations of cholera patients. Most of those to whom I have shown the specimens entertain no doubt on the subject, and all seem to concur in their identity of form.

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* In every opportunity that I have had of examining the intestines of those who died from cholera, these bodies have been found adhering to the mucous membrane in shreds of white matter, and very abundant; and the inference is, that in these very rapid cases they are in the intestines, though not given off in the evacuations.

This is all that is possible in respect to matters of such extreme minuteness, and we must, I imagine, be satisfied here, as in similar cases, to form our opinion on circumstantial and corroborating evidence. And when we consider that this form is in itself of too definite a character to be one of mere chance; that the sizes are progressive, accurate measurement showing those in the atmosphere to average from the 10,000th to the 3,000th of an inch in diameter; those in vomit from the 8,000th to the 5,000th; those in dejections from the 6,000th to the 500th; whilst they are met with in the same specimen of dejection in all the intermediate stages of palpably the same object;—the inference is, it appears to me, conclusive, that the annular bodies of atmosphere, vomit, and dejection, are but the three stages of development of one and the same body, of whatever nature it may be. A reference to fig. 3 will show them in almost all these stages, some being nearly as small as those in the atmosphere, others intermediate, evincing a commencement of compound character, whilst the largest again frequently seem parent cells containing young cells in their walls and interior, or broken up into a number of young cells more or less separated from each other. I have seen some much larger than the largest here represented, occasionally entire, but more frequently broken with a sharp irregular fracture, the morsels presenting in some measure the same characteristic annulus as the parent cell did. Their form is too peculiar to need further comment, the light ring round them giving them a peculiar cupped appearance, which is unmistakeable, especially to one who has seen the drawings.

Having thus given a detailed account of the mode in which I was led to the discovery of these bodies in the atmosphere and evacuations, it might be expected that I should enter more fully into a description of their nature; but, as I have stated in the commencement of this report, my object is simply to lay before the profession

the facts as I have found them, that they may receive the attention and examination which I believe they deserve, and be tested and proved by a repetition of my own experiments. To this end I shall be happy to give any information to those desirous of prosecuting the research, fully satisfied that the more they are examined the more fully will they be established, and the more important will be the results that may flow from their knowledge, and with the earnest hope that we may through them obtain, if no more, at least one common ascertained fact on which the profession may be agreed, and by which our inquiries into the cause and effect of this and other allied diseases may be directed in the right path.

On this account, also, I have studiously avoided giving any opinion at all on the facts brought forward, lest I might by so doing distract attention from them, and because I would wish them to stand alone as a fixed and demonstrated truth, from which others, as well as myself, may draw their inferences.

I have necessarily formed opinions, and been led by circumstances which occur only in the actual practical investigation of such subjects, to conclusions which may or may not be approved by others, and I should therefore wish these to be considered as totally distinct from the facts stated in this report, and shall probably make them the subject of a paper in a future number.

P.S.—Since the above was placed in the printer's hands, I have been kindly furnished with the following letter, from Mr Quekett, for publication. The opinion of so high an authority will bear, I am sure, great weight.

Clifton, September 21, 1849.

Royal College of Surgeons,
September 20, 1849.

My Dear Sir,—I have carefully examined the specimens procured by you from the air of cholera districts, choleraic vomit and evacuation, submitted for my opinion on Friday, September 14th; as also a specimen obtained from the atmosphere this day, and have no hesitation in stating that in my judgment they are successive stages of development of the same body, which I believe to be of a fungoid nature.—Yours very truly,

Dr F. Brittan. John Quekett.

2.—*Microscopic Characters of Cholera Discharges.* By Thomas Williams, M.D., Lond., Licentiate of the Royal College of Physicians, late Demonstrator on Structural Anatomy at Guy's Hospital.

On the 10th July of this year a solitary case of cholera occurred in this town (Swansea), which proved fatal, and which was attended by Mr Rowland and myself. It was a case imported from Shields. The discharges, which were quite characteristic, I submitted to careful microscopic and chemical examination. The singular polygonal confervoid bodies, of which figures accompany the present communication, were *then* clearly and repeatedly demonstrated and witnessed by my friend Mr Rowland. Faithful sketches of the objects which then arrested my attention by their singularity, and which contrasted so strikingly with the prismatic epithelium of the intestinal mucous membrane, were at the moment made in my notebook.

Early in the month of August the cholera rushed upon us in this town with great suddenness and virulence, and opportunities became abundant for testing what I had conceived on the 10th July to be a *new fact* in the pathological history of cholera. Fourteen cases were accordingly subjected to a rigorous microscopic scrutiny. In those cases in which the rice-water purging was only about to commence, I never failed to discover the perfect or unbroken pentagonal cells shown in fig. A.

These bodies consist of flattened polygonal (for the most part) pentagonal corpuscles, the interior of which is incompletely filled by a mass of molecules aggregated in a form approaching the sphere, and presenting in the centre a nucleolar pellucid point.

In the more advanced cases, the granular bodies, which appear as though they were destitute of involucrum, and which are exhibited in fig. B, constituted literally *the whole* of the solids of the discharge. The flakes, shreds, molecular deposits, and floating filaments, were composed of *nothing* but these capsuleless corpuscles, the capsules being ruptured by the endosmosis of fluid into the cell. The detached loose molecules, from their levity, floated in every part of the fluid, and seemed to exceed by very little the specific gravity of simple water. These bodies were visible under the microscope after complete desiccation, and constituted in that state an impalpable powder, which, from its literal weightlessness, would float in the atmosphere. I was thus led to conceive that these ultimate molecules were the real germs or *ova* of the mature choleraic cell shown at fig. A, and that *they* formed the veritable carriers on the wings of the atmosphere of the disease from one person and one locality to another.

This, in brief, forms the outline of the theory of cholera which had arisen in my mind on the foundation of the *facts* which I have just recorded. These views I took occasion many weeks since to explain to my friend, the Rev. Calvert Jones.

This hasty communication has been elicited by the announcement in your last, that Mr Brittan of Bristol contemplated the publication in your next number of the results of his observations on the fluids of cholera, conducted under the microscope. I hurry away this short and ill-digested communication, with the desire that it may appear as quite independent of the discoveries of others, of whose observations, in this remote part of the country, it cannot be supposed that I have heard a syllable.

In the next week's number I hope to supply you with a more lengthened account of the results of my own examinations.

Swansea, Sept. 23, 1849.

3.—*Note on the Presence of Peculiar Microscopic Bodies in the Discharges of Epidemic Dysentery.* By WILLIAM BALY, M.D., F.R.S., Physician to the Millbank Prison, and Lecturer on Forensic Medicine at St Bartholomew's Hospital.

The following imperfect account of some microscopic bodies observed a few years since in the mucous discharges of epidemic dysentery would not merit publication, were it not for the analogy probably existing between the bodies referred to and those described in last week's Medical Gazette, by Mr Brittan, as occurring in the rice-water dejections of Asiatic cholera.

After a severe epidemic of dysentery which occurred in Millbank Prison, in the year 1842, the disease frequently reappeared at intervals. In the course of the years 1844, 1845, and 1846, I availed myself of the opportunities thus afforded me of examining, with the aid of the microscope, the characteristic mucous discharges of patients affected with the disease, and at length my attention was attracted by observing in the mucus peculiar bodies, which differed in a striking manner from any of the ordinary elements of healthy mucus, or of the normal animal tissues, though some examples bore a resemblance to the ova of animals.

The bodies in question were generally circular in outline, were far more opaque than the mucous globules and detached epithelium particles amongst which they lay, and varied greatly in size. The smallest appeared to be globular or nearly so. The larger ones were for the most part collapsed or broken. But all alike had a thick wall, and a cavity containing globules, which were opaque and apparently destitute of nuclei. In the small ones the wall also appeared to be formed of similar globules of uniform size, regularly arranged. In the larger ones the structure of the wall was for the most part less evident. But occasionally one of very large size was seen, in which the conglomerate structure was remarkably distinct. Once or twice, too, there lay near to such a large compound body, a large empty vesicle, widely torn open, which I could not help suspecting had till recently formed its investment.

At the time these observations were made, I was inclined to regard the globular bodies in question as the products of diseased follicles of the large intestine. I arrived, however, at no definite conclusion as to their nature. But when Mr Brittan, on the 14th instant, kindly showed me his preparations, and the drawings by Mr Swayne, of the peculiar bodies which those gentlemen had simultaneously discovered in the rice-water dejections of cholera, I was at once struck with the similarity between the larger of those bodies and the collapsed and broken corpuscles of the dysenteric mucus.

On further investigation, I have perceived that there is a marked difference between the bodies observed in the two diseases. Those which are seen in the rice-water dejections, and in the matters vomited in cholera, are of smaller size and more flattened, present no appearance of compound structure in their early stage, and at no period of their development have this structure so marked as it is presented by the corpuscles seen in the mucus of dysentery. Yet in both there is the same gradual development, and the same endogenous mode of multiplication; and both are evidently of extraneous origin. They are clearly not the same bodies, but it seems to me probable that they are of similar nature. And if the opinion prove correct that the bodies discovered in the discharges from the alimentary canal of cholera patients are vegetable organisms of the lowest order (*fungi*), then I should have little doubt that these bodies in the dysenteric mucus also are *fungi*. They resemble, indeed, closely in many respects the individuals of the genus Protococcus. They want, however, the finely-cut outline of most vegetable structures.

I have failed to find the same objects in the bloody mucus discharged from the intestinal canal in two cases of sporadic inflammation of the colon. It becomes, therefore, a question whether they are not peculiar to *epidemic* dysentery, and of constant occurrence in it. This is a question I have myself at present no means of answering. If it were determined in the affirmative, the fact would afford support to the theory that the so-called *fungi* of cholera are the causes of that disease. If, on the other hand, it were found that such bodies are only occasionally found in dysentery, even of the epidemic character, it would be evident that they stand in no close relation to the disease, or at all events are not its cause; and the conclusion that cholera is produced by a parasitic fungus would be weakened. This question, however, regarding cholera,—a question, the importance of which cannot be over-estimated,—will doubtless soon be determined by other evidence. I have alluded to it only to show what I regard as the more important bearings of the observations I have related.

If the bodies described in this communication are not parasitic *fungi*, then it may, I believe, be safely affirmed that no such bodies exist in the discharges of dysentery.

28 *Spring Gardens*,
29*th Sept*. 1849.

4.—*Microscopic Examination of the Discharges in Cholera*. By THOMAS WILLIAMS, M.D., Londin., Licentiate of the Royal College of Physicians.

Since my last communication to the Gazette, which I accompanied by one or two illustrations, selected in a great hurry out of a large collection of sketches made from time to time during the past twelve months, demonstrating what I had ventured to interpret as vegetable organisms present in the discharges of cholera, time has been allowed me to find those which represent these bodies as they most frequently occurred under the microscope. In this paper I wish merely to state a few further particulars in relation to the microscopic history of these singular objects. I had availed myself of every opportunity afforded by post-mortem inspections to determine whether the epithelium taken from any part of the intestinal tract could, by disintegration, or some other modifying process, be changed into the appearances observed in the fluids of cholera patients.

The results of these investigations are contained in descriptions and sketches in my note-books, parallel contrasts being repeatedly drawn between them and the bodies so distinctive of cholera fluids, and the inference pressed that the latter could not have consisted of disintegrated or modified epithelial particles. The vomited and dejected fluids in every instance brought under my notice possessed a greater or less degree of tenacity, resembling a fluid holding gum, slime, or mucus, in solution. This mucosity, on standing, subsided to the bottom, leaving a supernatant stratum of clear fluid, presenting invariably a sp. gr. little exceeding that of distilled water, ranging from 1·005 to 1·010: but, when the vessel was placed in a room of moderate temperature, after an interval of twenty-four hours the mucosity would again ascend into the clear fluid, appearing to swell by growth or fermentation.

From a repetition of the observations, I became so convinced that there was in the evacuated fluids a peculiar yeast-like substance, that in showing the vessels to my friend Mr Jenkins, M.A., I observed,—" Here is, I am convinced, choleraic virus enough to inoculate the whole population of the town with cholera." The characteristic bodies were more certainly and in greater number detected in this gelatinous, mucoid substance, than amidst the heavier deposit composed of lithic and phosphatic crystals, and epithelial scales, occupying the bottom of the vessels. Under low powers, as the half and quarter of an inch (Powel and Sealand's), this mucous cloud resolved itself into a thick collection of pellucid molecules, which, in every instance, as shown in all my drawings, were more or less numerously interspersed by what gave the appearances of dehisced or emptied cell-capsules. The pellucid granules I interpreted as the progeny of the cells, the involucra of which were constantly discovered in the same specimen. Examined under Powel's one-eighth, an illustration taken from my note-book, the copy and the original having been shown to several gentlemen, affords an exact conception of the characters presented by this curious substance.

Of the mucous substance from the dejections of cholera patients, in the characteristic rice-water discharges, the figure sent represents the appearances under a high power.

The group is composed,—1st, of large compound cells, sometimes oblong, sometimes polygonal, and *not* frequently detected; 2d, of fragments of involucra, *lunated* and flattened; 3d, of granules or molecules, assuming under a high power a discoid character; 4th, fragments of cells like half-moons, or three-fourths crescentic in figure, surrounded by pellucid molecules, which under the highest powers assume the forms of transparent discs, and may be readily regarded as the early stage of the former variety; and, lastly, the compound cell, pregnant with secondary cells and moleculars, which, in consequence of mutual adhesion or mutual pressure, receives a flattened polygonal, and not unfrequently an oblong and oval outline.

My examinations of cholera discharges during the past summer have been very numerous, having attended no less than 200 cases of cholera and choleraic diarrhœa of various degrees of mildness and severity, In *every* instance I keep separate sketches of what I saw under the microscope; in every instance where the discharges were characteristic, the three varieties of cells above represented were detected; in some only the compound cells. In no instance in which the fæcal matter was natural could these peculiar bodies be discovered. But it never occurred to me to view these bodies as cryptogamic organisms, until I became persuaded that this slimy element of the choleraic fluids grew in amount, after the manner of the yeast ferment, when these fluids were preserved in vessels,—and not until direct examination convinced me that this "frothing" or increase of the mucoid substance could not have been due to any chemical change. The microscopic appearances which I have endeavoured to describe have been so constant and uniform in true choleraic evacuations, that the most cautious cannot refuse to admit the bodies described as peculiarly distinctive of cholera fluids. On the full admission, however, of the fungal nature of these organisms, the exact relation in which they stand to the phenomena of cholera; how the presence of a vegetable substance in the alimentary canal can give rise to a frightful rush of fluid from its parietes; in what manner scientific ingenuity can trace such disproportionate results up to such inadequate causes; in what the intermediate lesions consist,—in fact, the living pathology of the disease,—can only yet be fancifully conjectured. According to my examinations, the slight opacity in cholera discharges, occasioned by heat and nitric acid, very unlike that white coagulation denoting the presence of true albumen, is due to the changes wrought by these agents on the mucous substance already described.

Swansea, Sep. 25th, 1849.

P.S.—Since the above was written, the Gazette has afforded me the opportunity of acquainting myself with the gratifying results of Mr Brittan's and Mr Swayne's researches. Without the slightest desire to divide with them any honour which may attach to these observations, after a careful comparison of their descriptions and figures with my own, it has appeared certain to those who have made these comparisons, that the objects described are really the same, although perhaps the phenomena have not been similarly read; but it is impossible to deny to Mr Brittan the merit which belongs to the highest order of philosophical research. The calmness, completeness, and caution, which distinguish the spirit of his communication, must challenge universal admiration. To Mr Brittan alone is due the honour of having imparted *true value* to the discovery of vegetable organisms in the discharges by establishing their identity with bodies existing in external sources. The microscope has established a grand epoch in pathological medicine. Demonstration has taken the place of vague and baseless hypotheses in the study of a class of diseases embracing the most fatal and malignant to which the human body is incident.

5.—*On the Microscopical Pathology of Cholera.* By NICHOLAS PARKER, M.D., Lecturer on Microscopical Pathology at the London Hospital.

Dr Brittan's paper in the Medical Gazette of last week contains the announcement of an important discovery. He states that he has found in the dejections and vomit of cholera patients peculiar annular bodies, which are supposed by Mr Quekett to be of a fungoid nature, and that subsequently he succeeded in detecting in the air of districts where cholera was prevalent, germs and cells which are believed to be earlier stages of development of the same bodies. It is assumed, moreover, that these bodies are peculiar to the evacuations of cholera patients, and that they have some essential relation to the disease. These statements demand from the profession a careful and dispassionate examination. It especially behoves those who are conversant with microscopical researches at once to establish or disprove the fact advanced by Messrs Brittan and Swayne; for the conclusions drawn from the observed fact will (if confirmed) have an important bearing on the views generally entertained respecting the mode of origin and of prevention of cholera, and other allied epidemic and contagious diseases. The doctrine that certain epidemic and contagious diseases arise and become diffused through the agency of minute living organisms, is by no means a new theory. With various modifications it has been more or less prevalent for centuries. But it had become well nigh exploded, until attention was again directed to it in our own time by the able writings of Holland and of Henle. Various arguments have been adduced in its support; the more important are of two kinds, negative and positive.

The negative arguments are—

1*st*, The insufficiency of known physical causes to account for the origin and spread of contagious diseases.

Thus, to explain the cause of cholera, unwholesome food, and impure water, peculiarities of living, of climate, and of geographical situation, changes in the physical properties of the atmosphere, of its temperature, its hygrometric state, its electrical condition, and circumstances connected with the geological formation of countries, have in turn been invoked and abandoned. The Hindoo in the sultry plains of Bengal, who subsists chiefly on vegetable food, and the Russian, inhabiting a climate where the thermometer is many degrees below zero, and living almost exclusively on animal diet, have nearly equally suffered. Hill and plain, dry and moist situations, valley and mountain (as at Landour, 8000 feet above the level of the sea), have all suffered, though in very unequal proportions. We cannot in any of these circumstances find a clear and sufficient explanation of the cause of the disease.

2*d*, Chemical analysis cannot detect the presence of any deleterious agent in the atmosphere to which these diseases seem fairly referrible; and yet cholera, like typhus, influenza, and the exanthemata, is clearly dependent upon some wide-spread general cause. For it appears spontaneously in a district which had been previously healthy; it attacks simultaneously, or nearly so, a number of persons; it is migratory in its course, passing from one locality to another, and that frequently at a considerable distance; it attacks individuals of both sexes, of every age, and of every constitution.

Epidemic diseases, then, cannot be referred either to changes in the known physical properties of the earth, air, water, or food, or to chemical alterations of the ingesta and circumfusa (for the ozone theory seems to be quite disproved by the remarks in the Medical Gazette of last week); and hence they have been thought by some to be dependent upon a minute living organism, an animated principle of contagion. The following are some of the more important positive arguments by means of which this view has been attempted to be supported.

1st, The increase of these diseases is promoted, retarded, or arrested by means that promote, retard, or arrest the growth and development of such minute living organisms. Along the courses of rivers, more particularly where they form a delta, rarely at their source, in alluvial basins and in damp situations generally, wherever large masses of animal matter in a state of decomposition are allowed to accumulate,—as about sewers, and those plague spots, the metropolitan grave-yards,—there do we find cholera, typhus, and their congeners, to be rife, and there do they commit the greatest ravages; and these are precisely the circumstances which favour the increase and development of the lower organised forms.

2d, The poison which produces many of these diseases possesses a capability of increase when received into the body, reproducing itself at the expense of foreign organic matter, so that the effect produced is not at all in relation to the frequently small quantity of poison taken into the system. Like yeast, which is known to be a minute fungus, *torula cerevisiæ*, these poisons increase by assimilation. Just as a small quantity of yeast will suffice for the fermentation of a large amount of saccharine solution, and, growing, will increase a thousand-fold, so a single drop of small-pox virus will, when received into a living body, reproduce many drachms of a fluid endowed with like contagious properties. But no inorganic substance possesses such a faculty, and hence the reproductive power must be ascribed to a living organism, and most probably to a minute vegetable germ, or cell. This view is still further strengthened by the fact of the poison remaining latent in the system for a certain period.

3d, Minute vegetable parasites, or their germs, have been actually observed in connection with abnormal states of living plants, of animals, and of man:—By Unger, in the parenchyma of the leaf of *chrysomyxa abietis;* by many observers in different species of grain; by Meyen, on the body of vibriones; by Westwood, on the surface of the silk-worm of China; by Busk, on the surface of *dytiscus marginalis;* by Bassi and Audouin, on the surface and in the interior of the common silk-worm; by Laurent, in the albumen and vitelline membrane of the ova of *limax agrestis;* by Ehrenberg, in the *salmo eperlanus;* by Müller, Retzius, and Creplin, in pike and other fishes; and by Goodsir and Bennett, in the *cyprinus auratus*; by Henle and by Hannover, on the toes and integument of *triton cristatus* and *punctatus*; by Stilling, on the surface of frogs; by Rousseau and Serrurier, on the surface of *testudo Indica*, and in the abdomen of hens, doves, and many other birds; by Rayer and by Pappenheim, in the yolk of hens' eggs; by Müller and Retzius, in the respiratory tract of *stryx nyctea*, and of *falco rufus*; and by Reidhardt, on the surface of the lung of various birds; by Ecker, in a closed capsule contained within the abdomen of a raven; by Bennett, on the face of a mouse; by Langenbeck, in the nasal discharge of a horse with glanders; and by Remak, in the contents of the stomach and intestines of rabbits, oxen, sheep, and pigs.

In man, cryptogamic plants have been found growing on the skin and mucous membranes, both during life as well as after death, and they have also been detected in many of the secretions and excretions of the economy.

On the skin they have been observed by Remak, Gruby, Bennett, and others, in favus; by Gruby, in mentagra and *porrigo decalvans* (*Herpes tonsurans* of Cazenave); by Gunsburg, in trichoma; and by Sluyter and Simon, in *ptyriasis versicolor.* On the mucous membrane of the mouth their presence has been signalised in a form of aphthæ (muguet of the French), by Vogel, Gruby, and others; and by Hannover and Bennett in the coating of the tongue; on the fauces and œsophagus by Hannover; on ulcerated spots in the intestines by Langenbeck; in the posterior chamber of the eye by Helmbrecht; in tubercular cavities, and in sputa, by Bennett and Rayer; in the fibrinous coats of the small bronchial tubes which are expectorated in pneumonia, by Remak; in vomited matter, by Goodsir, as well as by Gruby, Vogel, and Busk. In all fluid dejections, no matter how produced, whether by typhus, dysentery, errors in diet, or purgatives, by Remak; and in the evacuations of cholera patients, by Boehm. In carious teeth, by Erdl. In milk, urine, mucus, and pus, by many observers. We have now to add to the list their presence in cholera evacuations, where they have been recently again pointed out by Messrs Brittan and Swayne; and in the perspiration of cholera patients, by Dr Cowdell and Mr Curme.

That such vegetable parasites are not unfrequently met with in diseased conditions of animals and man, appears to be unquestionable, and we have now to inquire,—

1*st*, Whether they or their germs exist in the fixed vehicles of contagions, and in contagious atmosphere.

2*d*, Whether they are invariably present in contagious diseases, and in those diseases only.

3*d*, Whether a casual relation obtains between them and the diseased state. The exanthemata frequently arise spontaneously, and still more frequently perhaps they are communicated by contact with the sick, or the air surrounding them. They are produced as well by a something mixed with the air, as by a something existing in, and emanating from, the bodies of the sick. The something mixed with the air, and the something emanating from the sick, may fairly be presumed to be identical. In the exanthemata the something which possesses the faculty of exciting the same disease in a person previously healthy, is known to be associated with certain fluids of the body, viz. blood, lymph, and pus, for the disease may be communicated by inoculation with those fluids. But the most careful microscopical examinations cannot detect anything differing from normal blood and pus, in the blood which placed beneath the cuticle causes measles, or the lymph which causes small-pox. Moreover, the constituents of blood and pus cannot traverse the atmosphere, and those substances must therefore act merely as vehicles of contagion. As for the principle of contagion itself, it completely escapes all our means of research, and its material existence cannot be proved at all; still less can it be proved to be a fungus. In the muscardine, a disease which prevails epidemically among silk-worms, and which is undoubtedly caused by a fungus, it results, from the researches of Audouin, that the fungus is invariably present, and in every stage of the disease: that it is always one and the same fungus, and that the disease may be communicated to healthy worms by inoculation. This is also the case in favus.

Is it so in cholera? are Mr Brittan's annular bodies always present? Mr Brittan himself states that "in very rapidly fatal cases these bodies are sometimes to be met with only in very small quantity, or are altogether absent." In the evacuation of a patient admitted to the London Hospital with cholera, which proved fatal in twelve hours, I could not discover these bodies. It does not appear, then, that this fungus is invariably present in cholera evacuations, and it seems particularly suspicious, that it should be absent precisely where we should expect to find it in greatest abundance,—that is, in the most rapidly fatal cases. I leave for future investigation the solution of the point whether the bodies found in the air, vomit, and dejections, are of one and the same nature. Nor do I purpose entering into any discussion as to the precise nature of the bodies themselves, a question which requires a more extended investigation.

Granting them to be, as stated, of a fungoid nature, different species of fungi have been found by Remak, in nearly all forms of fluid dejections, no matter how brought about, and the presence of a fungus, differing it is true from that described by Messrs Brittan and Swayne, in the stools of cholera patients, has been especially noted by Boehm in his work on the morbid states of the intestinal mucous membrane in Asiatic cholera.

4th, Does a casual relation obtain between these fungi and the diseased state? The foregoing considerations lead me to answer this question in the negative.

1st, The active principle of the *fixed* vehicles of contagion cannot be proved to be a fungus, and therefore analogy would lead me to predicate the same respecting that of contagious atmosphere.

2d, Even were fungi present, they must be proved to be *essential*, and not accidental; the cause of the disease, not its symptom; for fungi are present in various and most different states of the economy, often without giving rise to any particular symptoms.

3d, But these bodies do *not* seem to be invariably present in the evacuations in cholera: they cannot, therefore, be its cause.

4th, The presence of a few fungi does not serve in any way to explain the terrible symptoms of cholera. Nor is our knowledge of the essential nature of this mysterious affection thereby materially advanced.*

On the Cholera Sporules, by Mr Basham.

On the 27th of September, I was engaged in examining the urine of a patient suffering from symptoms suspected to be connected with oxalate of lime. The urine had been passed about twenty-four hours. While searching for the crystals of the oxalate, the appearance of some annular-formed cells or sporules, some with minute nuclei, attracted my attention. The field of the microscope presented these sporules among many crystals of the oxalate, and some epithelial cells and mucous globules. Struck with their appearance, I sketched them in pen and ink at the time of observation. On the following day, Friday, I first saw in your journal the figures of the cholera sporules illustrating Dr Brittan's paper. I was forcibly impressed with their similitude to those I had sketched the previous day; and, on returning home, I was able to verify this impression by a re-examination of a small portion of the urine still remaining in an unemptied phial. Desirous of examining the cholera sporules from some cholera fluid, I obtained the serous discharges of a male patient, aged 12, who was collapsed, pulseless, cold, and livid. In less than half an hour this fluid was under microscopic examination, and the appearances were similar to what I had observed in previous examinations of this fluid,—abundance of broken-down epithelium, mucus globules, and disintegrated fibrine, and a few bead-like aggregations of minute crystals of oxalate of lime. The appearance of these crystals in the serous discharges of cholera has been observed also by Dr Waldo Burnett (American Journal of Medical Sciences, July 1849). I could detect no cholera sporules, after a very patient examination of more than an hour. Four hours after, the appearances in the field of the glass were similar; one or two cholera sporules were now visible, and as successive drops of fluid were submitted for examination they became more numerous. Fourteen hours after, similar appearances to those first observed, with the addition of numerous prismatic crystals of the triple phosphate, and the cholera sporules, had become very numerous: they were more abundant in the shreddy membranous portions, to which they seemed attached.

Tuesday, Oct. 2d, fourth day.—Fluid in a state of most offensive decomposition; all trace of sporules had disappeared; much shreddy membranous debris, like broken-up and decaying fibrine; millions of vibriones in active motion; nothing else, after a very patient examination of many successive portions of the fluid, could be detected, but these animalcules, and the decomposing membranous structures. On Monday, Oct. 1st, I again examined the serous fluid from another cholera patient, and within the first three hours I could not detect any sporules in successive portions of the fluid: in about four hours they first became visible; in ten hours they were abundant, with many crystals of the triple phosphate; and on the third day they were displaced by the ordinary vibriones of decomposing animal fluids.

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* Medical Gazette, 19th October 1849. No. 1142, p. 668.

If these observations be corroborated, they will show that these cholera sporules cannot be the cause or essence of the cholera disease, although they may be its accompaniments; for they appear to be produced in other animal fluids unconnected with this disease, and they are not apparent in the cholera discharges until a certain period after the evacuation of these fluids from the body. Their presence in the fluid contents of the intestines after death proves nothing; for they may be developed there, just as they are developed out of the body, by the spontaneous change which the fluid itself is destined to undergo. Similar remarks will apply to the vibriones that have been mentioned as appearing in cholera fluids (London Journal of Medicine, Feb. 1849, and Oct. 1849); as if these animalcules were not common to all decomposing animal fluids. Urine of a healthy character, or containing a large proportion of urea, which in its decomposition generates carbonate of ammonia and other ammoniacal salts, does not appear to favour their development; but urine that contains much mucus, or albumen, or pus, will, if kept long enough, generate them in great abundance. They appear almost simultaneously with hydrosulphuric acid and its compounds; and all fluids that decompose into hydrosulphuret of ammonia, and its allied compounds, promote most rapidly the development of these vibriones.

Attention to the subject of sporules in organic fluids will, I think, soon detect an almost infinite number of forms; some, perhaps, produced and generated only at certain seasons,—others specially belong to certain animal or vegetable fluids.

I have this morning observed some sporules on urine that is strongly acid, sp. gr. 1·018, and abounding in stellated crystals of the uric acid. They are quite new to my eye as an urinary product. They are somewhat like the torulæ of diabetic urine, but they want the true confervoid character. They are oval cells, arranged by their long diameter in a bead-like form, with minute granules or cellules developing themselves from the surface and junction of the parent cells. I shall watch their growth; and, if further observations upon the subject of these sporules be thought interesting, I shall have much pleasure in reporting the result of my investigations.

W. R. Basham.*

17 Chester Street, Grosvenor Place,
October 6, 1849.

The next speculator on this subject, is Dr William Budd of Bristol, who publishes a tract to which we may afterwards direct more specific attention.* At present it is merely requisite to say that Dr Budd follows Dr Brittan, Mr Swayne, and Dr Williams, in ascribing the presence and ravages of cholera to the presence of fungous productions or organisms in the living body. The general inferences drawn by that gentleman, from numerous observations made on the discharges of patients labouring under and dying of cholera, are the following:—

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* Medical Gazette, 19th October 1849. P. 686.

"1. That the cause of malignant cholera is a living organism of distinct species.

"2. That this organism,—in shapes hereafter to be described,—is taken by the act of swallowing into the intestinal canal, and there becomes infinitely multiplied by the self-propagation, which is characteristic of living beings.

"3. That the presence and propagation of these organisms in the intestinal canal, and the action they there exert, are the cause of the peculiar flux which is characteristic of malignant cholera; and which, taken with its consequences, immediate and remote, constitutes the disease.

"4. That the new organisms are developed only in the human intestine.

"5. That these organisms are disseminated through society, (1,) in the air, in the form of impalpable particles; (2,) in contact with articles of food; and (3), and principally, in the drinking-water of infected places.

"The existence of peculiar objects in the 'rice-water' discharges of cholera, totally different in nature from any common morbid product cast off by the human body, and having all the characteristics of living organisms, may be ascertained by any one who will take the pains to examine, under the microscope, a series of well-marked specimens of this kind. These organisms abound most in what passes from the bowels, but are also met with in great numbers in the matters vomited; especially in cases in which spontaneous vomiting is a prominent symptom.

"In the matters vomited they are, however, according to my observation, not only much fewer, but also, as a general rule, much smaller, than in the fluid which passes downwards.

"The best specimens of 'rice-water' to select for the study of these beings are those which are of a dirty-white.

"In these specimens they are generally much more abundant, of a much larger size, and, indeed, in all respects in a higher state of maturation than in the purely white specimens.

"Single individuals are occasionally met with in samples of this dirty colour, so large as to occupy nearly a sixth of the field of the eighth of an inch object-glass. These larger organisms are filled with a vast number of granules, which appear of a yellowish-brown by transmitted light. It is, in fact, to the presence of a large number of such organisms, so coloured, that the dirty colour of these discharges is due.

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* Malignant Cholera: Its Mode of Propagation, and its Prevention. By, William Budd, M.D., Physician to the Bristol Infirmary. London, 1849, 8vo p. 30.

"Individuals of this large size are, as far as I have seen, only met with in great abundance in very malignant cases; and I have, consequently, been led to look upon the occurrence of discharges having this dirty white colour as of extremely bad augury."—Pp. 5–7.

Dr Budd thinks it indispensable to the corroboration of his inferences to give the following proofs:—

"That the organism here figured is in some way essentially related to malignant cholera, may be clearly established by the following considerations:—

"1. By the characteristics of the thing itself, which, by showing it to be possessed of a complex organization, having a definite mode of development, a specific form, and spontaneous powers of growth and multiplication, stamp it at once as a living being of distinct species.

"2. By its constant presence in cases of cholera, and its absence (with a few casual exceptions, in which a small number of stray bodies of similar character are found) in other diseases.

"3. By its presence in the discharges in such infinite numbers." —P. 8.

The foregoing documents and extracts will serve to show the manner in which the hypothesis now mentioned was brought forward and supported, by those by whom it was either proposed or espoused and adopted. It was not received entirely without opposition; and while several persons expressed doubts, one or two, such as Dr Grenville, opposed it, and denounced it as unfounded and inadequate to explain the phenomena. Microscopic observation, and the reasoning founded on microscopic observation, were nevertheless strong if not omnipotent; and the opponents were either few or little listened to. At length, however, the hypothesis was assailed by one of the microscopical inquirers; and as further investigation seemed to be required, errors which had pervaded the whole doctrine were pointed out.

The first indication that the correctness of the hypothesis is unquestionable was given, by Mr George Busk, at a meeting of the Microscopical Society, on Wednesday the 17th of October 1849. From a letter by that gentleman, sent on the 22d of October to the Editor of the Medical Gazette, and which we understand contains the correct exposition of his views, it appears that Mr Busk found that the fungous productions described by Mr Swayne as present in the discharges of cholera patients is a species of *Uredo*, not the same species as that found in brown bread, but of the same *genus*, being three times the size of the *Uredo* of brown bread meal, and oval, not round.

On the nature of the annular bodies described by Mr Brittan and Mr Swayne, Mr Busk did not at first speak positively. On the smaller annular bodies he refrained from giving any opinion, for the reason that he had not, in any case examined by him, happened to notice these bodies, or the objects supposed to represent them. The large annular bodies, by which he understands large, irregular, usually more or less broken corpuscles, with thick unequal walls of undefined and undefinable aspect, he regarded as being probably the remains of branny and farinaceous particles, taken into the system as food.

He subsequently found that the small annular bodies, or those considerably under a 1000th part of one inch in diameter, procured from patients at Birmingham, were in all probability altered blood-disks. These small annular bodies consist of flattened disks, or rather are rings, the area of which is filled with a thin expansion containing granules or minute spherules. Under a magnifying power of 600 linears, the ring presents distinct tranverse lines; and under a higher magnifying power, as 1200, it is very clearly seen to be divided into numerous almost moniliform segments. The corpuscles have the same average size as the blood-disk. Any fragments of these rings, when properly viewed, are seen to exhibit the same structure as the perfect rings; and therefore, whatever the nature of these bodies may be, their identity, Mr Busk conceives, cannot in future be matter of doubt or difficulty.

The corpuscles hitherto described as occurring in cholera discharges, Mr Busk divides into three orders, of which he gives the following characters:—

1. The larger, coarser, ill-defined annular bodies, or masses, are altered branny or farinaceous particles, or of that nature, meaning by bran, the inner layer of that substance.*

2. The more defined, uniform, and ovoid corpuscles, delineated by Mr Swayne, and regarded by him as typical forms of the cholera *fungus*, are the sporules of a species of *Uredo.*

3. The smaller diskoid annular bodies, averaging about a 3000th part of one inch in diameter, are altered blood-disks.

The opinion of Mr Busk on the etiological value of these phenomena is conveyed in the following passage:—

"The question with respect to the nature of the bodies under discussion, of course does not involve that of the 'fungous theory' of cholera in general; but with reference to this I would remark, that whatever its applicability to the mode of propagation or spread of the disease may be, the theory seems to me quite inadequate to explain its symptoms and course. To allude to merely one point: is it compatible with anything we know about the growth and development of fungi, to suppose that organisms of that kind should be propagated in such enormous quantity as to destroy life in an hour or less, without the generation of any gaseous product?"†

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* It is evident that by this is meant the inner layer of the coat or husk of the wheat-grain.

† Medical Gazette, p. 734, October 26.

Though this opinion was of considerable weight against the facts as specified, whether it affected the hypothesis or not, yet was it the opinion only of one person, and therefore it was quite competent for the advocates of the hypothesis to controvert its validity. Both parties, it may be allowed, examined under the microscope the same objects; and both parties observed the same appearances. The two parties, however, put upon these appearances different interpretations. One party maintained that what they witnessed were fungous productions, which were seen in the water used as drink in the places where the disease prevailed. Another observer interpreted these appearances as altered farinaceous matter, as sporules of spoiled grain, and as altered blood-disks. In this difficulty the Report from the Cholera Committee of the College of Physicians, a document remarkable for the temperate and distinct character of its statements, and the judicious and cautious expression of its deductions, comes to determine the question.

Report on the Nature and Import of certain Microscopic Bodies found in the Intestinal Discharges of Cholera.

The following Report was, in substance, laid before the Cholera Committee on the 17th instant; and we were then authorised to send copies of it to the members of the college. The epidemic was already rapidly declining, and it seemed desirable to make known the results of our inquiry before the opportunity of verifying them should be lost.

Dr Jenner's observations, and, with a single exception, Mr Marshall's, were included in the original draught read to the committee. On the same day Mr Busk communicated to the Microscopical Society his observations on the identity of some forms of the "cholera bodies" with a species of uredo, the contents of bran-cells, and starch-grains, respectively. These have been embodied in the Report, together with the result of Mr Marshall's examination of bran-cells (made subsequently, but independently of Mr Busk's), and the fact, that the smallest "cholera bodies" are derived from chalk,—a result to which we were led by the remarks of Dr J.W. Griffith on their physical and chemical characters.

William Baly.
William W. Gull.

We propose, in this Report, to lay before the Committee the results of some experimental inquiries on a subject which, within the last few weeks, has engaged much of the attention of the profession. We allude to the discovery, by Mr Brittan and Mr Swayne, of Bristol, of peculiar bodies in the "rice-water" dejections of cholera patients; and to the statement that similar bodies have been found by Mr Brittan in the atmosphere, and subse-

quently, by Dr W. Budd, in the drinking-water of infected localities.

These observations, on account of their important bearing, if true, on the pathology of cholera, seemed to us to demand a searching examination. We have, accordingly, given much time and attention to the subject. Having, in the first place, satisfied ourselves of the distinctive characters of the bodies found in the rice-water dejections, we next sought to verify the observations of Mr Brittan and Dr Budd with reference to their presence in the air and drinking-water of places infected with cholera. It was necessary that this part of the inquiry should not be delayed; for the epidemic had already reached its turning point, and it would before long have been difficult to obtain favourable opportunities for experiments of a satisfactory character.

Our inquiries were afterwards directed to the nature and properties of the newly-discovered corpuscles, and to the question of their occurrence in other diseases. In this investigation, we soon perceived that objects totally different had been regarded as identical; but we had arrived at no positive conclusion respecting those which seemed most characteristic of the cholera evacuations, when we received two important communications on the subject from Mr Marshall and Dr Jenner. The letters of these gentlemen are appended to this Report; but the results obtained by them are embodied in it.*

Our observations on the air and drinking-water of infected localities, twenty-four in number, gave uniformly negative results. With regard to the value of our experiments, taken separately, it will, we think, appear that many are liable to no objection. Some of those which relate to the drinking-water of infected places, are certainly wanting in the conditions which would make them convincing. But when it is considered that Dr Budd believes he has detected the objects sought for "in great numbers," in such large bodies of water as the Float, at Bristol, and the Surrey canal, and that he represents them as being deposited in the sediment of the water, we shall not be thought unreasonable in having expected that they *might* be discovered in the cisterns of houses and public institutions in which cholera has prevailed severely, although it had ceased there for some days or weeks.

Nevertheless, a much larger amount of evidence would have been required to disprove the statements to which our observations refer, had those statements been unassailable from other points. But the facts to be detailed in the subsequent part of this report, will show, that the bodies found in the rice-water dejections have no peculiar relation to cholera; and that, if they should occasionally be present in the atmosphere, or impure water, this will not happen exclusively, or even especially, in districts infected with the epidemic.

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* For the reasons here assigned, these letters are not given.

We shall now submit the particulars of all the observations to the Committee, describing first those on the air.

Microscopic Observations on water condensed from the Atmosphere of Infected Localities.

Two methods were employed for condensing the aqueous vapour. One was, to suspend in the air to be examined a glass funnel, nearly filled with a freezing mixture, its lower opening having previously been closed by a cork and covered with sealing wax. The moisture, condensed on the outside of the funnel, trickled into a small phial placed beneath. The second method was, to force air slowly, by means of bellows, through a bent glass tube immersed in ice and salt; when the moisture was deposited on the interior of the tube, and collected in a bulb at its lower part. In either way, from half a drachm to a drachm of water was readily obtained.

Obs. 1.—In Millbank prison, from the 6th June to the 16th September, there occurred eighty-four cases of cholera. The last patient began to suffer from diarrhœa on the 16th September, and died on the 25th. On the 19th, when he lay in a state of collapse, about a drachm of water was condensed from the air of a lobby which separated his small apartment from a water-closet, in which his evacuations were emptied. The water thus obtained was submitted to microscopic examination the same evening.

Obs. 2 *and* 3.—On the same day (the 19th September), we accompanied Mr Bayfield, one of the surgeons of the Union of St Olave's, Southwark, to two localities in his district, in which cholera had been most prevalent, namely, English Ground, Bull Court, Tooley-street, and Gimber's Rents, Snow's Fields. In a ground-floor room of a house in the former court, a woman and child who died of cholera within a few days; and the husband, at the time of our visit, was in bed, ill with the disease. Nearly a drachm of water was obtained from the noisome atmosphere of this room.

In Gimber's Rents the drainage and the ventilation were as bad as possible. In several places we saw the openings of drains covered with matting, to prevent the escape of effluvia. We collected about a drachm of water from a house where a woman lay ill of cholera; her husband having only recently died of the epidemic. The water procured in these two experiments was examined the same evening, and the examination of it repeated on several subsequent days.

From Gimber's Rents we brought away a piece of bread which had been long in the house, and which had not been cut for a week; a piece of butter, the surface of which was covered with dust; and a jug, which we found filled with drinking water. The examination of this water will be referred to in our second series of experiments. On the bread and the butter, no bodies like those observed in the rice-water evacuations could be found.

Obs. 4 *and* 5.—On the 22d September, water was condensed from the atmosphere in two houses situated in St Erman's Hill, near the Broadway, Westminster. Mr Painter, Surgeon of St Margaret's parish, to whom we had explained our object, conducted us to this locality, as, at that time, the chief focus of the disease. In one house (No. 21) a child lay dead, having been attacked with cholera the preceding evening. Two other cases had recently occurred in the same house. At No. 12 a child was ill of cholera; and a second had been removed, in the morning of the same day, to the Cholera Hospital, where he died. Mr Brittan and Mr Newport took part in the microscopic examination of the water condensed from the air in these houses, about an hour after it was collected; but, like ourselves, were unable to discover any "annular bodies." On the following day, the same water, as well as that procured in the second and third observations, was again examined by Mr Brittan, and with the same result.

Obs. 6.—On the 6th October cholera appeared amongst the patients in the insane ward of the Birmingham workhouse; many were attacked. On the 9th October, at our request, Dr Fletcher, of Birmingham, kindly obtained for us some water condensed from the atmosphere of this ward, and likewise from that of one above it, when diarrhœa was prevalent. These specimens of water reached us, and were examined by us, on the 11th October.

Obs. 7.—From the beginning of the month of October cases of cholera had been numerous and fatal in the workhouse of the Walsall Union; partly imported, but partly occurring in inmates of the workhouse. When the epidemic was at its height, we obtained, through the kindness of Dr F. Burton, of Walsall, about a drachm of water from the air of the room in which the greater number of the cases occurred. This specimen of water was condensed from the air on the 7th, and was examined by us on the 8th October.

The water condensed from the air in the several localities and under the circumstances we have described, was, in each case, examined by us more than once. But the search for "annular" bodies, such as those found in the cholera dejections, failed, as we have already intimated. Neither cells, nor rings, nor anything bearing a resemblance to them, could, in most cases, be discovered. We saw merely portions of gelatiniform matter containing bright points,—sometimes finely granular, brownish masses, perhaps derived from smoke,—and occasionally colourless, transparent particles, of a crystalline appearance, which may have been portions of silicious dust. After the water had been kept some time, chains of delicate oval vesicles, like those of the torula of yeast, but much smaller, appeared in it. These were absent at first, and could not be mistaken for the Cholera discs. Equally unlike those discs, were the three or four separate oval cells, which, in two instances, were seen in the water when first examined. They had a clear, single outline, and were not flattened.

Microscopic Observations on the Drinking-Water of Infected Places.

Obs. 8.—On the 26th September, Dr Snow kindly furnished us with a specimen of water from Albion Terrace, Wandsworth, a locality in which cholera had been very fatal between the 28th July and the 13th August. This water, which is very foul, had been taken from a tank at Albion Terrace on the 16th or 17th of August; but, as it had been kept so long before it was submitted to microscopic examination, it may be objected that, had "cholera fungi" originally been present, they might have become decomposed or otherwise destroyed.

Obs. 9.—A second specimen of water, sent to us at the same time by Dr Snow, was obtained from a house in Gresse Street, Rathbone Place, in which five persons had recently died of cholera. The last case of the disease here, occurred two days before the water was obtained from the cistern, and twenty-four days before it was examined by us with the microscope.

Obs. 10.—From the 31st August to the 7th September four fatal cases of cholera occurred amongst the female prisoners in two wards of the Millbank prison. The part of the building where these wards were situated was forthwith vacated. On the 19th September a portion of water with sediment was taken from the cistern which supplied those wards, the contents of this cistern having remained undisturbed since the removal of the prisoners to another part of the building. At the same time, specimens of water were taken from the cistern of the female infirmary, where two fatal cases of cholera had occurred simultaneously with those above referred to, and also from the tank which supplies the whole prison. These three specimens of water were submitted to the microscope the next day.

Obs. 11.—The drinking-water taken on the 19th September from the house in Gimber's Rents, Borough (see *Obs.* 3), deposited a sediment which was carefully examined. The description of the locality has been already given.

Obs. 12.—On the 27th September we visited Crosby Court, Bermondsey, an open space containing seven houses. Four cases of cholera (two fatal) had recently occurred in one of these houses, the others having escaped. The house in which cholera had been fatal was closed. But we found that the water used by the inhabitants of the court came from two pumps; one supplied from the Thames water, the other raising well-water, which was hard and ferruginous. On a strict inquiry it appeared that the Thames water was used for drinking in every house except the one in which cholera had appeared; in that house only the well-water was used. We brought away water from both pumps, and examined the deposits, which were abundant, on the following day.

Obs. 13.—We next went to Jacob's Island, Bermondsey, a most crowded and wretched part of the district, in which cholera had been very severe. It is a portion of low ground bordering the river, and surrounded by a shallow tidal ditch, which receives the contents of the privies on either bank. The water for drinking and other purposes, is taken for the most part from this ditch. We procured some water from the ditch itself, and also from two pumps supplied from it, situated in Gutteridge's Court, where deaths had occurred.

Obs. 14.—On the same day we also went to Hanover Street, Rotherhithe, a low and crowded cul-de-sac. A woman lay dead of cholera in one of the houses; and other deaths had occurred. The water used, of which we procured a specimen, was derived from the Thames, through a pump which became dry at every ebb. Behind the houses on the east side was an open ditch, receiving the refuse from them and conveying it into the Thames, at a point close to the opening of the pipe which supplied the pump in the street.

Obs. 15.—In a house in Swan Lane, Rotherhithe, close to the Millpond, a man lay dead of cholera. The disease had been fatal in two adjacent houses. The woman, whose husband had just died, told us that she and most of the inhabitants took their water for drinking from the Millpond, which is a tidal ditch serving as a sewer to the houses on its banks. We filled a bottle with water from this source.

Obs. 16.—Dr Burton of Walsall forwarded to us, on the 7th October, three specimens of water from the workhouse, cholera prevailing at the time amongst the inmates. (See *Obs.* 7.) The first was from an open cistern fed by land drains, the second from a moat, the third from cisterns in the workhouse. The last water, which is that chiefly drunk by the paupers in the workhouse, is derived from the moat, but is filtered through charcoal and gravel. The deposits of all were carefully and repeatedly examined.

Obs. 17.—To Dr Fletcher of Birmingham, we are indebted for five specimens of the water which supplies the workhouse, forwarded to us at the time cholera prevailed in the insane ward of the establishment (See *Obs.* 6). The specimens included:—1. Clear water from the reservoir which is supplied from the river; 2. Sediment from the reservoir; 3 Clear water from the cistern of the workhouse; 4. Sediment from the bottom of the cistern; 5. Sediment from the side of the cistern. These specimens were examined on the 11th instant, and the examination of them has been most carefully repeated.

Obs. 18, 19, 20, 21, and 22,—were all made on water obtained for us by Mr Hunt, one of the assistant-surgeons at the Westminster Cholera Hospital, from several parts of the district called Palmer's Village, which we have ourselves inspected. The names of the places are Goodman's Green, where the water used for all purposes is contained in a filthy open trough; Perrin's Place; a house (No. 3) in Perrin's Court, in which five cases of cholera (two fatal) had occurred; a house (No. 2) in Providence Row, where also there had been five cases (three fatal); and the Dispensary, Palmer's Village, at which two surgeons successively had been attacked with cholera, one fatally. Cholera had prevailed in all these localities, but not within a fortnight of the time when the water was taken for examination.

Obs. 23.—In a small house (No. 9) in Dorset Place, Vauxhall Road, three cases of cholera had occurred in succession; the first on the 5th instant, the second (fatal) on the 8th, and the third (also fatal) on the 12th. These cases were attended by Mr Clark, of St James's Terrace. On the 8th instant, just after the second case occurred, two specimens of water were, at our request, taken from the butt supplying the house,—one from the surface of the water, the other from the tap after the sediment had been stirred up; and both have been several times examined with the aid of the microscope.

Obs. 24.—On the 5th October, a man labouring under cholera was admitted into University College Hospital. He had resided for some months at No. 4 Howland Street. A week previous to his attack he had changed his room to another in the same house, where a woman had died of cholera seven weeks before. Water was taken from the kettle, and from a stone water jug in his room, as well as from a cistern which supplied the house. As he had dined in another house on the day previous to his attack, water was procured thence also. The deposits of these several specimens were likewise submitted to careful microscopic examination.

The uniform result of these experiments, as of the former series, was negative. No bodies were found which could be regarded as identical with the more characteristic of those discovered, by Messrs Brittan and Swayne, in the rice-water dejections of cholera. The objects met with were far more numerous than those seen in the moisture condensed from atmosphere. The sediment, when viewed with the 1-8th inch object glass of Ross, or 1-16th inch object glass of Powell and Lealand, presented, besides amorphous matter, an almost endless variety of organic forms, both animal and vegetable. Amongst these were many round or oval cells, of various dimensions, and some separate rings of minute size, colourless, and pellucid. The cells had generally very delicate walls and a clear cavity, were never flattened, and often contained a multitude of distinct granules, which, in some instances, presented the molecular motion. Like the rings, these cells were obviously different in their nature from the thick-edged disks, which the descriptions and drawings of Messrs Brittan and Swayne, and Dr Budd, had led us to regard as the characteristic corpuscles of the cholera evacuations.

The negative results of our search in the atmosphere of infected places, for objects identical with those just referred to, are confirmed by some observations communicated to us by Mr Marshall. While cholera was prevalent in St Giles's, he examined the dirt washed from the broken glass of windows, and from cobwebs taken from houses in that district, in which deaths had occurred from four to ten days previously. With 1-12th inch or 1-8th inch object-glass, he found a vast number of objects, such as particles of silex and soot, hairs, wings, and legs of insects, round and oblong cells of a brownish colour, very dark spherical granular masses, probably of a confervoid nature, and fragments of vegetable tissue, amongst which were pieces of spiral tubes, and entire rings, apparently of woody tissue, of an oval, polygonal, or circular form. But he detected no disks with double outline. A microscopic examination of the objects collected on a moist surface, from the atmosphere of sewers, gave Mr Marshall a similar negative result with regard to those discoid bodies; although he found (besides fine particles of silex and other dust) brown, oval, and round cells, single and in couplets, minute colourless vesicles, either single, double, or in triplets, a single large oval cell, and numerous opaque granular confervoid bodies, of a brownish or blackish-green colour.

Microscopic Observations on the Bodies found in the Cholera Dejections.

We next proceed to show how various are the bodies which have been confounded together under the terms annular bodies (Mr Brittan), cholera cells (Mr Swayne), and cholera fungi (Dr Budd).

On examining the drawings given by the three gentlemen who have called attention to the subject, four principal forms, which can hardly belong to the same objects, may readily be distinguished.

1. *Rings,* which enclose a free area, and which often are broken. These are usually of minute size, but occasionally large.

2. *Globular or oval cells,* chiefly of the middle size, which have a thick wall with numerous small eminences on its surface, and contain a granular mass, in some instances separated by a clear space from the wall of the cells. These are distinctly figured only by Mr Swayne, but are regarded by him as perfectly developed cholera cells.

3. Bodies having, apparently, the form of *discs, with thick rounded edges,* and centres of indistinct structure. These vary extremely in size, including some of nearly the smallest, as well as many of the largest, of the objects represented by the three observers (Mr Brittan, Mr Swayne, and Dr Budd). They predominate in all the representations given of the corpuscles of the rice-water dejections, and must be taken as the type of the bodies discovered by Messrs Brittan and Swayne.

4. Large *broken cells,* having apparently homogeneous membranous walls, and containing small, well-defined, oval bodies;

figured by Dr Budd as cholera fungi undergoing decay, but differing in character from all the other objects represented.

A mere inspection of these different figures would suggest strong doubts as to their representing different appearances of really identical bodies in different states or stages of development or decay. The more particular description we have now to give of each kind of body, will demonstrate that they are of various and distinct nature.

1. The rings, when closely examined, are seen to be of different kinds, some perfectly continuous in their entire circle, others formed by a curled fibre; some round, some oval, others lozenge-shaped. Some of these have been traced to their true source by Mr Marshall, who has found that exactly similar objects may be prepared by the artificial digestion of the vegetables used as food; such as cabbages, potatoes and onions, the withered style of wheat grain, and portions of cane in sugar; the spiral and annular tissues of which break down into rings of different sizes, or coils resembling rings.

Many minute oval or round colourless corpuscles, which have an annular appearance, are, on close inspection, seen to have their area filled up with a transparent substance, presenting sometimes perforations. In some specimens of the rice-water fluid, oval bodies, in part having their middle filled up as here described, and in part mere rings, exist in extraordinary abundance. The rings of these bodies have been observed, by Mr Busk and Dr Griffith, to be divided by cross lines into segments, which Mr Busk thinks are bead-shaped: an appearance which had occasionally been noticed by ourselves, as well as by Mr Marshall. They are calcareous structures, originally derived from chalk, in which they abound; and they have been introduced into the contents of the intestines with the medicines (chalk-mixture, aromatic confection, &c.) which the patients have taken.* These minute bodies from the chalk are, of course, not found in all cases; and we think it not unlikely that, in their absence, the separated nuclei of animal and vegetable structures, as well as the vegetable rings above described, may sometimes have been mistaken for fungi. Fig. 12 represents a portion of mucus from the stomach of a cholera patient, where the nuclei of epithelium are seen becoming freed from their surrounding cells, and assuming more and more of an annular appearance. Fig. 13 shows the green nucleated globules of a conferva, with some of the colourless nuclei free, and presenting a distinctly annular appearance, as they were seen in some drinking water from Dorset Place. (*Obs. 23*). The microscopic bodies from chalk are, however, the most striking in their characters, and, we believe, are those which have been chiefly regarded as representing the first stage of the cholera fungi. They may be distinguished from those consisting of vegetable rings by the action of dilute nitric acid, which dissolves them, but has no action on the latter bodies.

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* It is right to state how we arrived at the knowledge of these facts. Dr Griffith had pointed out to us that the bodies in question are heavy, polarize light, are soluble in dilute nitric and muriatic acid without effervescence, are not destroyed by incineration, and are unaffected by iodine or potash. Hence there was no doubt as to their inorganic nature. He believed that they were oxalate or phosphate of lime. Mr Marshall subsequently showed us that acetic acid also dissolves them readily, and that sulphuric acid acts on them, producing needles of sulphate of lime. Having ourselves found the same bodies in the evacuations of two patients suffering from typhoid fever, we were examining them in company with Dr Griffith and Mr Marshall, when the demonstration of their calcareous nature reminded us of the fact, that these patients had been taking medicine containing chalk, and, at the same time, brought to our recollection the remark made to one of us by Mr Topping, that Mr Brittan's "annular bodies" were to be found in chalk-mixture. Accordingly, we examined a portion of medicine containing aromatic confection, and afterwards a piece of common chalk, and in both found the bodies described above, though not the larger disks which are also found in the rice-water fluid. Ehrenberg figures these calcareous bodies, and describes them as being "crystalloids." Abh. d. Akad. d. Wiss. z. Berl. 1838, p. 68.

2. The globular bodies have been clearly identified by Mr Marshall with the spores of different kinds of Uredo, the rust, smut, and bunt of grain; some species of which may be found, not only about the withered style on grains of wheat, but also in almost every specimen of corn and bread.

Mr Busk has made the same observation; and identifies them with the *uredo segetum*, or bunt.

3. Disks, with thick, elevated, and somewhat irregularly-curved margins; the central area flattened, and obscurely granular. They have generally a yellowish, or pale-brown tint, which varies in depth with the colour of the fluid containing them. These are the most peculiar of the bodies found in cholera, and differ from the rest in being more or less soluble in ether. Mr Marshall, who first informed us of this fact, found that the smaller disks undergo nearly complete solution, leaving a cavity in the dried mucus, whilst the larger ones leave a fine granular film. They are apt to break across, and the thick margin to curl inwards. They are evidently not cells, nor have they any organized structure which could give them any claim to be regarded as living organisms. On the other hand, their solubility in ether shows that they consist in great part of some substance of the class to which the fats, resins, and saponaceous matters belong. This observation led Mr Marshall to examine different fatty substances, and at length to find that curled concretions, not unlike the disks found in cholera, could be obtained by compressing a piece of rich cheese (with or without the addition of ether) between two plates of glass. We are not yet able to account for the origin of these peculiar disks. Mr Busk regards the smaller ones as altered starch grains. It is at all events certain that they are not fungi;

and, as we shall afterwards see, that they are not peculiar to cholera.

Mr Busk thinks that the larger disks are the altered contents of bran-cells. Mr Marshall, too, has, independently, made the observation, that certain yellowish bodies, sometimes seen, which have a thinner and narrower border than the fatty disks, and are merely rendered pellucid by ether, may, perhaps, be derived from bran. The granular masses contained in bran-cells have, however, when undigested, no distinct border. Fig. 18 represents their appearance when acted on merely by potash.

4. Under the fourth class of bodies, we refer to those represented by Dr Budd as the cholera fungi undergoing decay and disintegration. They are evidently of a different nature from those figured by him as characteristic of the fresh cholera dejections. The mode of disintegration of the two classes of bodies is quite distinct: the so-called cholera bodies, after resisting the action of water for some time, break up into irregular granular masses; whilst the decomposing bodies depicted by Dr Budd seem to be in part homogeneous, membranous cells dehiscing; and are, perhaps, starch cells. The rings are, most probably, parts of disintegrated vegetable tissue.

It is shown by Mr Marshall, and had before been noticed by Boehm and others, that cells like fungi, or their spores, are occasionally found in the excretions in cholera. These, however, have a more delicate structure than any of the bodies described as characteristic of cholera, and are totally different from them. It is well known that various vegetable forms are apt to become developed in organic fluids generally.

From a review of the foregoing facts, it is obvious that various bodies found in cholera dejections have been confounded, and described as identical. It is also shown that many are traceable to an extraneous source, and that even the disks placed in our third division are not fungi. The statement, that the bodies found in the cholera dejections present an endogenous multiplication, has, in all probability, arisen from confounding them with the uredo, or from mistaking the appearances produced by the small bodies seen through or upon the larger ones, or entangled in their substance.

We are unable to identify the rings obtained from the air and figured by Mr Brittan with any of the bodies included by him under the term "annular bodies." Our own experiments have satisfied us that these bodies do not commonly exist in the atmosphere of infected places; but the observations of Mr Marshall on the dirt collected from windows and cobwebs, show the great variety of matters which must be wafted about in the air, in the form of dust, and which might, in different instances, be caught with the condensed moisture.

The bodies represented by Dr Budd, as being found in impure drinking water, have the form of disks with thick edges. We have ourselves never seen such bodies in water. But if it should be established that the contents of bran-cells sometimes assume that form, the occasional presence in water of bodies capable of being confounded with the disks derived from the discharges of cholera, will not appear remarkable.

Had the bodies described by Messrs Brittan and Swayne been proved by the foregoing investigations to be of fungoid nature, yet the facts we have now to add would have shown that they have no necessary connection with cholera. In the first place, they seem not to be constantly present in the discharges. It is, indeed, remarkable that in those dejections which, from the absence of colour, have usually been regarded as the most characteristic of the disease, they are frequently absent. We have failed to find them in several instances. In one, a portion of every evacuation was set apart, and examined several times by each of us, and yet in no portion could we detect them.

A still more important fact, which, from the explanations already given might be anticipated, is, that all the more remarkable of the bodies which have been thought peculiar to cholera, exist in the intestinal evacuations of persons affected with other diseases. Dr Jenner first demonstrated to us their presence, in great abundance, in the dejections of a patient affected with typhoid fever. We have since verified his observation in five other cases of this disease. We have also satisfied ourselves of the existence of some of the forms in dejections apparently healthy, from two patients in Guy's Hospital, one suffering from bronchitis, the other from early cirrhosis of the liver; and Mr Marshall has detected small annular bodies "in the mucus covering the healthy excrement" of several herbivorous animals. It is obvious that bodies derived from such various sources will not commonly be found all present together. This, indeed, is not the case in cholera. The minute bodies, especially, which belong to chalk, will, of course, very rarely be met with, except that substance has been taken as medicine.

We shall now briefly restate the principal results we have arrived at, and submit the conclusion which seems to us justified by them.

1. Bodies presenting the characteristic forms of the so-called cholera fungi are not to be detected in the air, and, as far as our experiments have gone, not in the drinking water of infected places.

2. It is established that, under the term, "annular bodies," "cholera cells," or "cholera fungi," there have been confounded many objects of various and totally distinct natures.

3. A large number of these have been traced to substances taken as food or medicine.

4. The origin of others is still doubtful, but these are clearly not fungi.

5. All the more remarkable forms are to be detected in the intestinal evacuations of persons labouring under diseases totally different in their nature from cholera.

Lastly, we draw from these premises the general conclusion, that the bodies found and described by Messrs Brittan and Swayne are not the cause of cholera, and have no exclusive connection with that disease;—in other words, that the whole theory of the disease which has recently been propounded, is erroneous as far as it is based on the existence of the bodies in question.

(Signed) WILLIAM BALY, M.D. } *Cholera*
WILLIAM W. GULL, M.D. } *Sub-Committee.*

The facts contained in the foregoing report will enable any one moderately acquainted with the rules of weighing evidence to form his own judgment as to the validity of the hypothesis which ascribes the origin of cholera to *fungi.*

In many respects the report cannot fail to be useful. It will show the necessity of greater deliberation and more careful examination of facts, and, above all, of greater caution in the interpretation of microscopical appearances, before they are applied confidently to sustain any new hypothesis. It seems to be a current belief with many microscopical observers that whatever is seen is fact. The truth, however, is, that objects seen by the microscope are not in all instances facts. They are only appearances: and, to interpret these appearances right and without error, requires both a practised eye and an acute and instructed mind. It is only when the correct and unerring interpretation is put upon these appearances that they can be said to be converted into facts, and admitted as facts. This process is accomplished with different degrees of facility in different cases. In some instances much varied examination is necessary; in others less is required. In some the process of interpretation is so easy, that it can scarcely be said to exist. In others it is so difficult, that the most practised and dexterous observers entertain the most opposite views as to the indications to be formed from the same appearances. Of this discordance in the process of interpretation, numerous examples have occurred at all times in the history of microscopical observation; and, during the last ten years, when microscopes of the most perfect construction have been in use, they have not been diminished in number. Between the two extremes now specified, lie all varieties of facility and difficulty of interpretation; and, in probably the great majority, the tendency is rather to difficulty than to facility, and consequently to the favouring of discordance in the deductions drawn by observers, and the application of these deductions to the theory and practice of medicine. It is almost superfluous to say that, of this difficulty and the consequent differences in opinion and deductions, the fungous hypothesis is, if not a good example, at least one sufficiently in point.

Granting, however, that all the facts were correctly and indisputably established, as stated by the advocates of this hypothesis; allowing that it were proved that, in the discharges from the bowels of cholera patients, sporules and fungous productions were frequently or constantly found,—there still remains a considerable interval between such facts and the inference that cholera depends on the presence of fungous organisms in the human body. Fungous organisms are present in other conditions of the body, and do not produce cholera. Fungous organisms may be present, and may be the effect of cholera; they may be present, and be the effect of the state of the system in which cholera takes place; and they may be present, and be the effect of another agent which produces cholera. Upon all these questions it is manifest that research and further experience alone can throw light; and, until that light is obtained in sufficient quantity, and adequately concentrated, to dispel all doubt and uncertainty, and to obviate all differences in opinion, it is assuredly not an unreasonable demand by the advocates of true science to require that the hypothesis be considered as an hypothesis merely, and not as a generalized induction, or a well-founded theory. Admitted as a hypothesis, the speculation may be useful in promoting inquiry, in collecting facts, in distinguishing facts from opinions and inferences, in weighing the force of evidence, and, in short, in forming a solid foundation for the erection of a theory, or in showing that the foundation is altogether sandy and unstable, and consequently unfit for the construction of any theory. All these are beneficial results. Received as a theory, the speculation produces nothing but hollowness, uncertainty, errors, and delusion.

Since the publication of the Report of the Cholera Committee of the Royal College of Physicians, Dr Swayne has addressed to the Editor of the Medical Gazette, a letter, dated 27th October, regarding the statements and inferences of Mr Busk. The latter gentleman had forwarded to Dr Swayne, specimens of the bread in which he found sporules of *Uredo;* and Dr Swayne allows that this bread contains an *Uredo* identical with one species from wheat, (*Uredo caries*), of which he had previously obtained specimens.

Dr Swayne then states, that he found upon examination many points of difference between the *Uredo* and the bodies contained in cholera evacuations; but he looked in vain for any striking points of resemblance. The chief points of difference are, that the cholera cells are larger than those of the *Uredo* (twelve lines); that the cholera cells vary in size, are oval, and are met with in every stage of development; that the cholera cell contains a ball of granular matter, like a yolk ball or vitellus, not completely filling its interior; that the walls of the cholera-cell are proportionally much thicker than those of the *Uredo;* and that the cholera-cells are completely though slowly dissolved in nitric acid.

Dr Swayne allows that he and Mr Brittan met with bran-cells, as also starch-cells in the evacuation; but the cholera corpuscles he maintains that he carefully distinguished from these.

The small annular bodies he denies to be blood-disks; and he states that these bodies are observed, when made to roll over, to be globular. It is well known that blood-disks, unless much altered, retained their flattened diskoid character.

Like other microscopical observers, he repels the insinuation, that he and Mr Brittan had fallen into mistakes in consequence of using high powers and bad illumination.*

Dr Swayne makes no mention of the facts contained in the Report from the College of Physicians, nor of the inferences deduced by the authors of that report.

It is easy to perceive that in this state the question will not remain, though it is not difficult to see that the partizans of the organism hypothesis will not readily give up what they conceive to be their facts. Questions in science cannot, like questions in civil polity, jurisprudence, and political economy, be decided by the number of votes. This question, nevertheless, will probably be determined by a judge seldom mistaken in his decisions;—Time. This of course implies more extended and careful research, freedom from bias, and,—shall it be said?—from prepossession; and probably the additional light progressively thrown on the subject by different inquirers. To that tribune we must, in the mean time, leave the question.

ART. VII.—*On the Nervous Papillæ of* VATER; *the Corpuscula of* PACINI. By Dr J. CARL STRAHL (Muller's Archiv., Heft ii., 1848, Seite 165).

ON the nature and anatomical signification of the Pacinian bodies, at three different times, accounts more or less full have been given in this Journal. The first was, when their existence was first formally announced by Pacini, the author of the discovery of these bodies, at the meeting of natural philosophers and physicians at Pisa in October 1839. This was in our fifty-third volume, p. 268, 1840. The next was an abstract of sundry observations made by

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* Medical Gazette, Nov. 16, p. 862.

Pappenheim, in our sixty-seventh volume; (April 1847.) And the third was a short abstract of the facts ascertained by Henle and Kolliker, in their memoir of 1844, in volume sixty-ninth; (January 1848).

The following essay by Dr J. Carl Strahl furnishes some additional facts.

Henle and Kolliker gave, in the year 1844, in an express monography, a more accurate description than previously had been given of the anatomical structure of those peculiar terminations of Nerves which are found in the cutaneous Nerves of the extremities. In the beginning of their Treatise, they give a historical report upon the discovery of these peculiar corpuscles. According to this sketch, Pacini had discovered these bodies only in the eighteenth century; and, for this reason also, the name of *Pacinian Corpuscula* has been given them in science. Nevertheless, one hundred years before the appearance of the monography now mentioned, these bodies were delineated as terminations of the nerves in the hand and in the foot. The somewhat rude figure in the dissertation by J. G Lehmann, *de Consensu partium Corporis Humani. Vittembergae,* 1742, *2do Novembris*;* is made from a preparation by Abraham Vater, which is preserved in the museum at Wittemberg. From this preparation, which has lain for a long time in spirit, of course no conclusions on the minute structure of these *Corpuscula* can be drawn, and an unfortunate accident happened, as the author states, to a newly-made preparation.†

If, however, these *Corpuscula* are to be named after their discoverer, they ought henceforth to be denominated Vater's *Corpuscula*, although Vater applied to them the name of *Papillæ*. In the title of the dissertation by Lehmann, these bodies are called *Papillæ Nerveæ*; but in the text, and also in the twelfth and last thesis, in which only they form the subject of discussion, they are called indiscriminately *Papillæ cutaneæ*, and *Papillæ nerveæ*; and it appears almost from this, although nothing is stated on the function of these *Papillæ*, that Vater had considered them to be peculiar organs of tact or sensation.

Anything which, after the forgetfulness of a whole century, recent observers of these bodies have said on their functions, is not very satisfactory. They appear not to be connected with sensation, and particularly with the sense of tact, because, when they are met with in the extremities, they are found exactly at spots at which this function is in no wise expressed with peculiar energy.

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* Haller's Disputationis Anatomico-Physiolog., Tom. ii.

† The accident was, that a dog entered the dissecting-room and devoured the preparation without being perceived till it was entirely eaten. The figure in the Dissertation of Lehmann, though rude, gives a very distinct idea of the figure and general characters of these *corpuscula*.

Often, indeed, for example, they are found in greater amount in the *Nervus digitalis volaris secundus;* but the greatest number are then found in the neighbourhood of the ball of the thumb, in which situation I found them also in January 1842 without knowing them, while, in the tips of the fingers, they are almost entirely wanting. They are also imbedded in great number in the subcutaneous cellular tissue, and rarely project into the *cutis vera.*

The presence of the Vaterian *Papillæ* in the Nerves of the mesentery of the cat suggested to Pacini the idea, that they might have some connection with the development of electricity. Henle and Kolliker, however, made a mistake, when they, in order to establish this hypothesis by experiment, subjected the bodies in question to the action of Bohnenberger's Condensator, since avowedly, with this instrument, it is possible to measure the electricity of dry bodies only, not of humid animal tissues. In order to determine a question so important in two senses, I performed experiments with the galvanic Multiplicator; but from these the result was, that in the Vaterian *Papillæ* no electricity can be demonstrated.

For these experiments a cat was put to death by strangulation; the abdomen was immediately laid open, and the Pole of the wire of the Multiplicator was applied to both sides of the Vaterian body. Not the slightest vibration, however, took place in the magnetic needle, whether we placed the pole in the long axis, or in the broad axis of the Vaterian body, or caused the pole to enter the substance of the body. Actions of these bodies on other organs, could in general not be established; I could touch them freely with caustic potash ley, or crush them, or puncture them, without in any mode observing in remote spots, or in the neighbourhood of the bowel, any more violent action, or exciting any convulsion in other muscular parts. The function of these bodies, therefore, remains still involved in obscurity, although the idea, that they might, probably, have a relation to the electrical currents in the nerves,—deduced from their diffused presence in cats, animals abounding in electricity,—has considerable probability, if we consider the peculiar structure of these bodies.

On this occasion I may be allowed to subjoin, on the structure of the Vaterian *Corpuscula*, some remarks, which I have derived from observations many times repeated. However careful are the researches made by Henle and Kolliker upon the minute structure of these bodies, in which they surpass those made by Mayer, still all has not yet been accomplished.

I shall here speak first on the entrance of the nerves into the Vaterian *corpuscula*, and then of the extent of their capsule, and their structure.

The primitive nerve-fibres, which enter into a Vaterian corpuscle, very speedily undergo peculiar morphological changes. While without the Vaterian corpuscles, the nervous fibre which very quickly shows under the microscope those changes which are ascribed to coagulation of the nervous medulla, is tightly enclosed by neurilema, the latter is in many instances already detached in the neighbourhood of these bodies, yet for the most part, first at that point at which it enters the corpuscle, and runs into the peduncle. At the last spot I have frequently enough seen, in the most distinct manner, the formation indicated in the figure which I have published, and which appears as if coverings crossed the nerve-fibre, from the outer coverings in the substance of the corpuscle. It seems to take place in this manner. The neurilema is separated or reflected from the nervous fibres in almost regular layers, and thereby forms round it a sheath with bead-like enlargements. In some instances these reflections are indistinctly marked; but when they are large, they appear, if we reason from optical phenomena, to be filled with fat, which is deposited free, without being enclosed in cells, and communicates from one reflection to another. This free mass of fatty matter, then, is in connection with the central cavity; at least I have never been able to observe a partition-wall, although the nature of the content of that cavity appears to be entirely different from it. Something similar, if we judge from his figure, Mayer appears to have observed.

In the neurilema, which penetrates into the peduncle, I have not been able to recognize any fibrous structure so distinctly as Henle and Kolliker represent it. But I have in the most decided manner satisfied myself that this structure terminates with the peduncle in sharp, abrupt boundaries, and never enters along with it into the central cavity.

The peduncle or stalk itself, notwithstanding the multiplied inflections which it makes, lies almost everywhere in one and the same level; and in few cases only does the observer require to change the focus of the microscope in the slightest degree, if he wishes to observe in their entire length the nervous fibres running in the corpuscle. So much more striking, on the other hand, is the change of focus, which becomes requisite, if we trace them into the central cavity; the level of the nervous fibres in the central cavity manifestly deviates from that in the peduncle, and appears to be more in the axis of the whole Vaterian corpuscle. At the spot at which the nervous fibre penetrates from the peduncle into the central cavity, this fibre is apparently considerably elevated, in consequence of which it becomes difficult at this point to observe it accurately. This apparent alteration in the level of the nerve-fibre, however, depends only on the dioptric phenomena of the convex capsule-walls, and the contents lying between them; because the observer, in proper turning of the Vaterian corpuscle round its axis, can observe no corresponding inflection in the nerve-fibre. The system of the internal capsule makes on one side a greater curvature to the peduncle, and the amount of the same following each other in rapid succession, produces a difference so much the more remarkable, in comparison with the central cavity. Meanwhile I have never been able to observe, that at those spots at which the nerve-fibre appears slightly bent, the latter gave off anything from its elements, and it entered the central cavity only in an altered form. This circumstance is so much more unexpected, as the nerve-fibre is smaller in the central cavity than in the peduncle, and also shows distinctly a flattened aspect; but the observer can demonstrate nothing whatever of its elements left behind; and if he open the entire capsule under the microscope, and strip it off from the central fibres, he perceives how the latter, which was previously only slightly outlined, becomes now progressively doubly outlined, and presents the wonted characters which have been long known to pertain to nerves. The peripheral end of this nerve-fibre is, nevertheless, when we exert no pressure on the Vaterian corpuscle, not so distinctly knotted as the early delineations represent. Meanwhile the club-shaped terminal swelling begins in all cases after pressure, and when the observer has cleared the nerve-fibres from all investing capsule.

These and the subsequent observations were made on the Vaterian *Corpuscula* from the mesentery of the cat. In the hot days of summer, and especially when they are moistened with water, they lose their entire structure. In such circumstances I could preserve the subject for the microscope longer, if I employed acetic acid instead of water.

The capsules, which give the Vaterian corpuscles their peculiar shape, vary much in number and width. Usually there exists, as the early observers represent, a system of internal capsules; nevertheless, this does not always contain a like number of capsules; and the latter are not in all cases smaller, as the outer, but often interrupted in consequence of their breadth. In other respects these internal capsules differ not from the external ones; so that to their difference little importance can be attached.

The capsule-walls consist of structureless ligamentous tissue, in which are imbedded *nuclei*. A fibrous structure I myself have not been able with strong magnifying powers to recognise; and at most I have only observed a difference of longitudinal and transverse fibres. In this it is also natural, that I may not have seen the admitted fibres of the capsule-walls at the peduncle continued into the neurilema; since all the capsules are here bounded in the most definite manner, and in a definite form. For here, at the central poles, all the capsules are something expanded or unfolded, the inner as well as the outer, when they proceed upon the peduncle, and are rounded off in a peculiar manner. This progressive enlargement is partly the cause that here the diameter of all the capsules, from the central cavity outward to the surface of the Vaterian body, is larger than at the peripheral pole. Besides this, the observer still perceives running in the peduncle transverse lines, which proceed both over and under the nervous fibres. Thus two arched lines are blended on both sides of the corpuscle, and proceed most accurately to the capsule-walls. These last lines are the transverse sections of the capsules with the peduncle; and there accordingly remains no doubt, that the capsules at the central pole are perforated by nerve-fibres and their neurilema;—a fact which is valuable as to the outer and the inner capsules. This arrangement is most distinctly seen in the second figure, which representation, if an actually observed variety, is particularly instructive, on account of the small number of capsules. Whether this object be a corpuscle taken in the process of development, I shall not say.

Each capsule consequently is closed within itself, and has no communication externally. The capsules, however, do not all proceed from one side to the other; since many, judging especially from this circumstance, that those at the central pole are broad only in one direction, proceed to the peripheral pole. To these capsules, nevertheless, similar capsules on the other side may correspond. On the peripheral pole only about the half of the capsules run over from one side to the other. For here, indeed, very often two capsule-walls are blended with each other, and then proceed onwards as one single wall. Hence it results that, at the peripheral pole, the space from the central cavity outwards is narrower than at the central pole, or any other spot of the Vaterian *Corpusculum*. This same blending of parts is, further, the reason why the anatomist cannot strip all the capsules at the peripheral pole, and finds at this point an intimate connection; a phenomenon, for the explanation of which Pacini had recourse to the idea of peculiar intercapsular ligaments.

This, however, is not the sole cause of the small diameter of the peripheral part of the Vaterian corpuscle; but some of the most internal capsules are here also perforated by the central cavity. The observer finds here, in short, similar transverse lines to those of the peduncle, which are intimately connected on both sides with the capsule-walls. Besides this, the observer also perceives the capsules thus perforated terminate accurately in the central cavity, without their proceeding round and across to the other side. Often also have I seen some of the inmost capsules terminate in the central cavity, before they had yet reached the peduncle.

In conclusion, I have only to remark that most commonly one particular nerve-fibre runs into each Vaterian body. In one instance, further, I observed that one simple nerve-fibre is divided, and each portion terminated separately in one corpuscle.

Art. VIII.—*Researches on the Acidity and Alkalinity of Certain Liquids of the Human Body, in the state of Health and Disease.* By M. Andral. (Annales de Chimie et de Physique. Troisieme Serie. Tome Vingt Quatrieme. Paris, 1848. P. 116.)

The different animal fluids considered in their physiological state, all present a certain degree of acidity or alkalinity. Some transitory influences may accidentally render them neutral. Thus the introduction into the stomach of a large quantity of water may in a rapid manner deprive the urine of its acidity. Thus also, when the skin is covered with a very abundant perspiration, this fluid, which is naturally acid, may cease to be so, and may present itself in the neutral form. But it is manifest that in each case the acidity of these fluids disappears only because their acid principle is accidentally diluted in a large quantity of watery vehicle.

In the physiological state no spontaneous modification of the organism is permitted to convert a liquid naturally acid into an alkaline liquid, and the converse. If this transformation is occasionally observed, this depends, as the transition to the neutral state, on influences foreign to the organism. Such in certain circumstances is the operation, either of articles of food or drink, on the decomposition of certain liquids, either in the air or in their reservoirs.

It may then be established as a principle, that in man, when in the state of health, whatever be the varieties of his physiological condition, each of the different liquids of the body preserves uniformly the same reaction, alkaline for some, acid for others; at most they may sometimes become accidentally neutral, when a large quantity of water is introduced into the blood, or when, with the occurrence of the circumstance now mentioned, the liquids are secreted in much greater abundance than usual. This is particularly remarkable as to the case of cutaneous perspiration; which shows that, when the latter is increased to such a degree as takes place during sweating, the augmentation of the cutaneous perspiration has the effect of removing from the blood proportionally a greater amount of water than of other principles; for, if this did not take place, and notwithstanding its increase in quantity, the matter of the cutaneous perspiration would remain at all times acid.

When man is attacked by disease, the question is naturally suggested, do the fluids preserve the same sort of reaction as in the physiological state? can those fluids which are in the state of health alkaline, become acid in consequence of disease, and reciprocally? This question must be placed among those to which a definitive answer has not yet been given. It has been often repeated, and it is an opinion generally entertained, that to a certain number of diseases is given the power of modifying the fluids to that degree, that those which are naturally acid become alkaline, or those which are alkaline become acid. This opinion, brought forward at different periods of science, has been employed to support more than one theory upon the proximate cause and the nature of diseases; it has led to the establishment of symptoms to recognise the presence of certain diseases, and it has been employed to deliver several therapeutic precepts. It therefore appears to me not without importance to subject to the inquiry a new examination, and to learn if, in diseases, and by their influence, the two reactions, acid and alkaline, of the human fluids, are susceptible of being modified by being converted into each other.

Of all the liquids in the economy, the serum of the blood is that which has always appeared to me to present the strongest alkaline reaction. The intensity of this action appears not to vary sensibly, whatever be the nature and the duration of the disease.

It has been said that, in the instances in which the blood became very poor in fibrin, the proportion of its alkaline principles was increased; but the facts adduced in support of this opinion are as yet too few to permit us to ascribe to them great value. It has been said also, that in saccharine or true diabetes the alkaline element of the blood was diminished; but, to my knowledge, no analysis has been cited in support of this important statement.

May it happen in certain instances, that the blood loses its alkaline reaction? This question I propose, because a respectable author, Vogel, in his Pathological Anatomy, has recorded on the authority of Scherer, the case of a woman labouring under metro-peritonitis, in whom Scherer states that he found perfectly neutral the blood which had been drawn from the vein in blood-letting. Vogel, who relates this fact without denying it, and even without discussing it, remarks, nevertheless, that he has never observed anything similar. For my part, I have to say that the alkaline condition of the blood is in my eyes a general law, to which hitherto I have found no exception. As to the instances of which Vogel has in like manner spoken, and in which the blood was found acid, I cannot admit their validity. He proceeds, in his further discussion of the subject, without saying that my negation applies only to cases in which the blood examined was that of living individuals. Vogel, indeed, affirms that he has sometimes found the blood acid after death; but this acidity was then the effect of decomposition which the blood had undergone; it was no longer a fact of disease.

The liquids which are formed from the blood are seldom found in the neutral state. Most commonly, either they continue alkaline, like the blood from which they proceed; or they present an acid reaction more or less decided. This is what takes place in the physiological state. I now proceed to inquire what change the interposition and presence of disease may communicate in this respect to these fluids.

Previously, however, it is necessary to establish one fact. This is, that at the most of the surfaces of the body, external and internal, there are arriving at one and the same time several liquids, which most frequently present different reactions; so that, if the inquirer be apprised of this circumstance, he might commit a mistake by ascribing to a change in reaction in one of these liquids that which depends solely on the accidental predominance of the other.

For instance, the skin secretes two materials with different reactions; one is acid,—it is the perspiration; the other is alkaline, —it is the sebaceous matter.

Whatever be the conditions of health or disease, in which I have examined the perspiration, I have found it most commonly acid, sometimes neutral, never alkaline.

I have specified above in what conditions I have established the neutral nature of the perspiration. It is when it is extremely abundant. No disease removes its acidity; by no disease is it rendered alkaline. In typhous fevers, however severe they may be, the acidity of the perspiration continues. Nor is it true that this quality disappears in true diabetes, a disease in which, further, there are more frequent opportunities than is usually supposed, of being convinced of the properties of the perspiration; for in diabetic patients the cutaneous perspiration is often augmented; and I have seen diabetic patients, who, when arrived at a very advanced stage of the disease, presented, either in the course of the day or during the night, very abundant sweatings, though they were not labouring under pulmonary tubercles.

The skin, nevertheless, does not everywhere present an acid reaction; and in several points, even where it is covered with perspiration, it may exhibit a reaction perfectly alkaline. These points are those in which are numerous sebaceous follicles; for instance, on the nose in some persons, and more generally in the hollow of the arm-pits, over the eyebrows, and in various other parts provided with hairs. It is certainly not the perspiration which in these parts requires peculiar properties; that is not the secretion which becomes alkaline. It is the fat matter contained in the follicles which, in parts of the skin in which it is abundant, produces this reaction. This phenomenon, nevertheless, is not constant. While it is very well marked in certain individuals, it is not observed in others; and, further, it is present or absent independently of every special condition of health or disease.

Perspiration, then, is not simply the water of the blood which has escaped through the skin loaded with a greater or less amount of the principles of the serum. If such were the nature of the perspiration, then it ought to be alkaline, as is the serum of the blood, and as are the greater number of the liquids which are separated from the blood at the surface of the skin. Thus the liquid furnished by a portion of the skin, which has been irritated either by a burn or by the application of a common blister, or that produced by strong hartshorn, always presents a marked alkaline character. The fluid contained in the vesicles of herpes, or of ekzema, or in the blebs of pemphigus, is in like manner always alkaline. In all these instances, in which a process of congestion more or less intense precedes the exudation, it must be admitted that it is the serum of the blood which, modified only as to the respective proportion of these elements, is expelled from the vessels, and is effused on the free (epidermal) surface of the skin. There is, nevertheless, one vesicular eruption which is distinguished from all others by this circumstance, that the appearance of the vesicles is preceded by no symptom of congestion, and that it is the first and the only appreciable pathological element. This is the eruption known by the name of *sudamina*, a species of miliary eruption. By a remarkable exception, the liquid of the *sudamina* differs from that of all the other vesicular affections of the skin in this circumstance, that, instead of being alkaline, it is, on the contrary, remarkably acid; and in it we find no trace of albumen, while this principle is met with in all the others. The fluid of the *sudamina* is, therefore, the product of a peculiar process quite different from that which causes the other vesicular eruptions. This liquid, in its acid reaction, and its want of albumen, entirely resembles sweat. Often also, in the state of disease, we see *sudamina* produced in individuals who have very profuse sweats; but this last circumstance is not the sole cause of the occurrence of this form of eruption; for, in many instances of typhoid fevers, we observe multiplied *sudamina* covering the skin of the trunk, of the neck, and of the extremities, without sweating perceptibly taking place.

On the mucous membranes, in a degree still greater than on the skin, we find almost in all instances at the same time liquids of different kinds, and usually of different chemical reaction. Hence arises a certain difficulty in discerning, amidst this association of liquids, the reaction which belongs to each; hence, also, proceed the chances of error which have not in all cases been avoided.

In all parts of their extent, the mucous membranes in their healthy state furnish like the skin an acid principle. This principle exists in the transparent and globule-less fluid which these membranes separate from the blood in their physiological condition. But if, as is frequently the case, there be substituted in the place of this liquid an opake matter with globules, the acid reaction disappears, and its place is supplied by a well-marked alkaline reaction. Thus we find uniformly alkaline the opake mucus which is so readily secreted by the mucous membranes, as soon as they have become the seat of an acute or chronic inflammatory process. Few liquids, for example, are so strongly alkaline as is the puriform mucus furnished by the nasal cavities in cases of coryza. In bronchitic attacks, the matter expectorated presents sufficiently often united the two species of reaction, acid and alkaline; the portions of this matter which remain transparent are acid; those which have become opake are alkaline; and those two reactions are observed to remain perfectly distinct beside each other.

The mucous membrane of the mouth, including that which covers the two surfaces of the tongue, presents a reaction, which is not at all times the same. If examined in the morning, before any food has been taken, it presents in the great majority of cases an acid reaction; but in the course of the day this condition changes, and becomes alkaline. The first sort of reaction belongs to the matter furnished by the buccal mucous membrane; the second belongs to the saliva. It is, therefore, a mistake to say that the acidity of the mouth is owing to a morbid state of the stomach, and particularly that it denotes the presence of inflammation of the stomach. Acidity of the mouth is not a pathological fact. It is observed in persons in the best possible health, in those who digest in the most healthy manner, and it may be found in the most opposite diseases. It disappears as soon as a certain quantity of saliva is caused to flow into the mouth; it is observed to be much more strongly marked, in proportion as the period at which it is examined is longer after that at which food has been taken; and hence it is easy to understand how this reaction is stronger and more enduring in diseases, in which for a certain time a system of strict diet has been observed.

Thus, therefore, the liquid which is separated from the mucous membrane of the mouth, is, in the physiological state, acid; and such it remains, in all possible forms of the pathological state. In those instances in which the mouth shows itself to be alkaline or neutral, that is not because this liquid has changed its nature. It is because its secretion has ceased, or because its chemical reaction is disguised by that of another liquid, which derives its origin not from the mucous membrane.

When, after death, a slip of turnsol paper is applied over the mucous membrane of the mouth, most commonly we observe that this paper is very decidedly reddened; sometimes it remains blue; but never has the gastric mucous membrane presented to me any alkaline reaction. As to the acid reaction of the stomach, I have observed it, both in the instances in which the stomach contained the remains of food, and in those instances in which for a long time no digestion could have taken place. By what means are we to reconcile these facts with others furnished by experimental physiology, and the result of which is to show, that the stomach would evince acid reaction only when stimulated by the presence of food or of different foreign bodies, while, when it is empty, it presents no alkaline reaction whatever? This is not what I have found to be the case in the human body. The various morbid states, in the midst of which patients are overtaken by death, have not appeared to me to induce any modifications in the nature of the reaction of the stomach. I have found it acid in the most different disorders; in typhus fever, in acute inflammations of the lung, in pulmonary consumption, in albuminuria, in true diabetes. This same acid reaction is further found, in a manner very nearly uniform, in the matters rejected from the stomach in the act of vomiting. There are among others few substances, which communicate to turnsol paper so decided a red colour as the black matter formed by the blood, which is so often vomited by patients labouring under a cancerous affection of the stomach.

Further, it is sufficiently common to find in the human subject after death, an acid reaction in the mucous membrane of the duodenum and in that of the jejunum. Nevertheless, as in this portion of the alimentary canal, there are received from the liver and the pancreas, fluids of alkaline characters; it is not very uncommon to observe this species of reaction in the duodenum, and even below that bowel. In the large intestine, I have in all instances ascertained a well-marked alkaline reaction.

I have yet to examine the nature of the reaction presented by the fluids secreted by several of the glands.

The tears have appeared to me to be uniformly alkaline. The same is true of the saliva. It has been stated, that in certain conditions of disease the saliva might lose the alkaline element, which constitutes its normal character, and might become acid. I think myself entitled to conclude, from my researches on this subject, that this result never takes place, and that to no disease is given the power of converting the saliva into an acid liquid. I have already said that in many persons, whether in health or under disease, the mouth presents a most decided acid reaction. This species of reaction has been erroneously ascribed to the saliva. It is easy to prove that this acid reaction is not the effect of the saliva, by introducing into the mouth any sapid body; under the influence of such body a quantity of saliva flows rapidly into the mouth, and from that moment we find in the cavity of the mouth a well-marked alkaline reaction. This fact shows that in this instance it is not the saliva which is acid; it is the liquid which is furnished by the mucous membrane of the mouth. It is, therefore, manifestly a mistake to assert, that in inflammations of the stomach the saliva becomes acid. An error is in like manner committed, when it is asserted that in diabetic patients the saliva acquires acid properties. Often, unquestionably, in diabetic patients, we find in the whole mouth an acid reaction; but this phenomenon is not peculiar to diabetes; and no more in this disease than in others does the acid reaction depend on the saliva. In order to be certainly convinced of this fact, I have caused diabetic patients who presented this reaction to chew a little of the root of pellitory; by this means I have produced, in a few instances, an abundant flow of saliva; and I have found, without doubt, that this liquid preserved its ordinary alkaline character. Thus falls to the ground one of the principal arguments which speculators have employed in order to support the hypothesis, according to which the formation glycosuria is regarded as the product of the acidification, either of the blood, or of other fluids of the animal economy.

In the state of health, the urine, when it has not remained too long in the bladder, and if it be examined shortly after it is voided, is always acid. This acidity, however, may become very feeble, or even may disappear, and be succeeded by a neutral state, if a very large quantity of drink has been introduced into the stomach, and if at the same time a copious diaphoresis do not take place. Under the influence of this latter process, the acidity of the urine is increased in a remarkable manner.

Some accidental circumstances may, in a person in good health, render the urine, for the moment, alkaline. Thus it may become alkaline in consequence of the introduction of water, loaded with alkaline salts, into the stomach; it may acquire alkaline properties during the use, more or less lengthened, of a diet exclusively vegetable. The privation of food, however long continued, does not deprive the urine of man of its acidity. But a remarkable circumstance is, that we see in some convalescents the urine become transitorily alkaline, at the time when we begin to allow them to take food.

In diseases, the numerous modifications which the urine undergoes in its composition, do not deprive it of its acidity; and if it loses this character, it is in consequence of influences of a character quite special, which I shall immediately explain. How multiplied soever have been my observations on this point, I have yet to find an instance in which, by the influence of disease alone, the urine passes from the kidneys in the state of an alkaline fluid. To me it is evident, that there is an error in the observations of those persons who have stated, that in typhoid fever the urine becomes alkaline. Already has this assertion been controverted by M. Rayer; and in his work on Diseases of the Kidneys we find it stated, "that having investigated the nature of the reaction of the urine in fifty cases of typhoid fever, not one was found, in which it had become alkaline." My own researches have led to the same result. Whatever be the form which the disease has assumed, however intense might be its severity, and even in its most advanced adynamic period, I have at all times found the urine decidedly acid. In those cases even in which the urine had remained a long time in the bladder, and in which the urine examined by me had been withdrawn by the catheter, it retained most usually its acidity. The opinion that in severe fevers the urine becomes alkaline, appears to me rather to have been delivered under the influence of certain hypothetical ideas than from any attentive observation of facts.

We read in several authors, that the diseases of the spinal chord possess the power of modifying the secretion of the kidneys in such a manner as to render the urine alkaline. In this respect these authors have manifestly confounded two different things. When in a person labouring under a disease of the rachidial marrow, the bladder remains sound, the urine which it contains enters it in an acid state, and is discharged from it in like manner acid. But if, on the contrary, the mucous membrane of the bladder has become the seat of a purulent secretion, the urine then is changed within this receptacle, and there becomes alkaline. This phenomenon, however, takes place sufficiently often in proportion to the frequency of affections of the bladder, at a period more or less advanced of diseases of the spinal marrow. The affections of the bladder are, in truth, the only morbid condition which I have seen render the urine alkaline; not that, however, which proceeds from the kidneys, but that which is contained within the bladder. The change which the urine then undergoes is a phenomenon entirely chemical. Placed in contact with purulent matter or other morbid products furnished by the bladder, the urine is decomposed and becomes ammoniacal. Further, purulent matter itself, whatever be its source, is a fluid in all instances alkaline; we find it acid only in some instances in which it has become changed in consequence of prolonged exposure to the air. The constancy of the alkaline character of purulent matter is, further, a consequence of its nature. What is purulent matter in truth except serum of the blood, in the midst of which are spontaneously developed special globules? Purulent matter, therefore, must in all cases be alkaline at the moment of its formation, as are always both blood and the different morbid serous fluids.

Thus, therefore, the different fluids of the animal economy exhibit, in the nature of their reaction, whether it be acid or alkaline, a degree of uniformity infinitely greater than might have been imagined. Amidst the varied modifications which the state of disease communicates to the composition of these fluids, disease possesses not the power of changing their mode of reaction, and in all instances they proceed with like properties in this respect, from the apparatus which separates them from the blood. The unchangeable character of the secretion of the alkaline and acid principles of the animal fluids is therefore a law of the physiological state, as well as of the pathological state; and the preservation of this law must be very important, since it continues, without exception, modified only in the case of the urine, in a temporary manner, by certain influences depending on dietetic conditions.

ART. IX.—*An Experimental Inquiry into the Effects of Hydrocyanic Acid, produced upon Animal Life.* By THOMAS NUNNELEY, Esq., F. R. C. S. E., Surgeon to the Leeds Eye and Ear Infirmary, and Lecturer on Anatomy in the Leeds School of Medicine. (Transactions of the Provincial Medical and Surgical Association. New Series. Vol. iii. 1847). Abstract.

THE deadly effects and the rapid operation of hydrocyanic acid upon animal life have been long known to the profession. Its employment in the treatment of various painful and unmanageable diseases,—namely, pulmonary consumption, certain diseases of the heart, certain affections of the stomach, some nervous affections, and some varieties of neuralgia,—have all contributed to render the knowledge of its powers generally known to the public. The occurrence also of various fatal accidents in instances in which it was administered by the direction of physicians have tended to the same results, and have made it be justly regarded as an agent of great power over the vital functions. The energy with which it acts, has recommended it for purposes both of murder and suicide; and, while in not a few instances fatal effects have taken place under professional or pretended professional administration; peculiar circumstances have tended to invest with obscurity the question of the mode in which it operates, and whether it is possible in all cases to distinguish its effects, either in the mode in which death takes place, or in the appearances afterwards found in the dead body. In short, though it is generally known that hydrocyanic acid is a most energetic agent,

and which operates with extreme rapidity, yet regarding the mode of its operation precise information has been hitherto deficient; and among medical persons, otherwise well educated, much discordance of opinion prevails.

The trials of Belany and Tawell, in 1844, were sufficient to prove these inferences; and since that time, occurrences have taken place tending to establish the same conclusion. Meanwhile the agent continues to be employed, sometimes for suicide, sometimes medicinally, yet from particular circumstances causing fatal results. Four years ago, 12th February 1846, a gentleman, of considerable eminence in the profession, destroyed himself by means of this agent; and on the 30th of October 1849, another professional gentleman committed suicide in the same manner.* In February 1847, a person, styling himself a physician, had prescribed with other medicines this remedy, to a young female under his care; and whether the remedy was of excessive strength, or the patient had taken too much, or the physician had committed a mistake, the patient died on the 15th of February speedily after taking a draught containing hydrocyanic acid.†

All these circumstances taken together, invest this substance with peculiar interest, and render it important to know correctly the peculiar symptoms, if any, by which its operation in the living body is attended; to discover the antidotes, if any, by which its effects might be counteracted; and to ascertain if there be any certain and unequivocal proofs derived from the symptoms during life and the appearances in the dead body, to show that this acid had been the cause of death, and that death had not been the effect of natural causes.

A considerable source of perplexity in all instances in which death has taken place in connection with the administration of hydrocyanic acid, is this. It is often impossible, in a certain class of cases, to tell whether the poison has been taken accidentally or intentionally. The individual may have been taking hydrocyanic acid by the direction of the physician; he may have taken several doses previously, without apparently more than the usual physiological effects; and all at once, after some particular dose, he is found frothing at the mouth, convulsed, speechless, senseless, and never recovers; or he is found quite lifeless. Secondly, the individual may have been taking the acid occasionally to allay irritation or excessive cardiac action, or with the idea that when he is in what is called a nervous condition, this will quiet him. He takes it; and some time after is found lifeless. Thirdly, an individual may be observed in an irritable and excited state, as it is called, with restless glaring eye, and unsettled look, not sleeping, peevish, fretful, easily put out of humour, offended with slight accidents;

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* Mr Thomas Morton.

† Edinburgh Medical and Surgical Journal, vol. lxvii., p. 598. April 1847.

and at some particular time afterwards he is found dead, with a phial of hydrocyanic acid not far from him.

In the last case little doubt can be entertained that the individual swallowed hydrocyanic acid intentionally. But in the first two cases nothing is less certain than this conclusion. It may be, that in both or in one of the instances, the individual, distressed by the symptoms of his complaint, or otherwise deprived of the power of reason, may have had recourse to such a dose of the agent as would end all his troubles in this life. But it may also be the case that, from some mistake on his part, or from some variation in the strength of the preparation employed, he, without any criminal intent, may have accidentally taken a fatal dose. It has never been very satisfactorily ascertained whether Mrs M'L. (L. E. L.) really committed suicide or died from the effects of an overdose of the medicine. It has been often said that she did take the medicine with the intention of destroying herself; and in one of the London periodical publications, this was formally stated; and reasons derived from domestic circumstances were assigned to explain the act. But it has been also asserted, that she was in such a state of ill health, that she might have died in consequence of a very slight overdose, not taken with the intention of its proving fatal. Within the last eighteen months a medical practitioner in one of the provincial towns was found dead in the morning, with his dress still unremoved, evidently after having taken hydrocyanic acid. But it was never distinctly ascertained whether he had intended to commit suicide, or had taken the remedy with the hope of alleviating some degree of nervous irritation, to which he was at certain periods and under certain circumstances liable.

Now, though a similar, or even the same degree of ambiguity, may have attended cases of death after the use of opium, it is certainly much more frequent in proportion, in cases of death after the use of hydrocyanic acid. Of this ambiguity various explanations may be given. But it is enough to notice the fact.

Hitherto hydrocyanic acid has been employed most generally perhaps under the circumstances specified, that is to say, of certain, or probable, or doubtful suicide. Mr Nunneley thinks that it is likely to be extensively employed in the perpetration of murder, as extensively as arsenic itself. On this point it is not easy to form any probable conjecture or satisfactory inference. But the circumstances already brought to light in the commercial and social history of this poison are such as to render it an object of very considerable interest, both in a physiological and a medico-legal point of view.

It is chiefly in consequence of the circumstances now adverted to, and the difficulties with which the subject of death, under the use of hydrocyanic acid, is necessary or contingently attended, that Mr Nunneley has been led to undertake an elaborate and skilful examination of the subject by the aid of experiments. It is probable, as is above suggested, that several of the difficulties are natural and necessary to the subject. Others may be accessory and contingent only. It is desirable to know the one class from the other, and to understand, if possible, the reality and the extent, as well as the causes of both. To accomplish these objects, the memoir of Mr Nunneley is well adapted. The inquiry is deliberate, carefully conducted, ample in information; and if he has not solved all the difficulties, he has made a nearer approach than any other inquirer to this consummation. In one respect the information furnished by Mr Nunneley is most serviceable. He shows that, regarding the effects produced by hydrocyanic acid on the animal body, there are prevalent several popular errors.

The experiments performed by Mr Nunneley are in number 152. They were performed on warm-blooded animals, as dogs, cats, and rabbits; among cold-blooded animals on various fishes; on spiders, on earth worms, and slugs; and in a considerable number of plants. On dogs the number of experiments is 80.

It may be stated, that during each experiment an assistant was present, who, like himself, was provided with a watch. He was placed at a table with pens and paper, so that each observation was recorded at the moment it was made. This will account for some of the expressions used. Some may think the time is stated with unnecessary precision and minuteness. But it must be borne in mind, that the exact period at which effects are produced, is not only one upon which there is the greatest contradiction, but which is one of the most important. In all cases where the strength of the acid employed is not mentioned, it was Scheele's. This was selected, as that most generally met with in this country, and consequently of the strength most likely to be employed in poisonous doses by the public. It also is of a convenient degree of dilution. The acid used in many of the experiments was obtained from Allen and Co., Plough Court; in the remainder, from Gale, Baker, and Co., Bouverie Street. In both cases it was recent; and from both sources it was of the exact strength of Scheele's acid, viz., five per cent. of real acid, and Mr Nunneley did not observe any difference in the effects. The strong acid Mr N. made by distilling at a low heat ten drachms of Scheele's acid, until two drachms had passed over into a receiver kept constantly as cold as possible. This, therefore, should be equal to twenty-five per cent. of real acid, or four drops to one of anhydrous acid. Throughout the following pages Mr Nunneley has purposely not made any reference to the experiments of others, not that he undervalues them, but his object was to experiment for himself, and not merely to repeat those of others; besides this, it was not until some time after the whole of those upon the dogs had been made that he had seen the account of the longest set of experiments at that time published,—those of Dr Lonsdale.*

The following abstract presents a summary of the general inferences which result from the inquiry of Mr Nunneley.

Four dogs were placed upon the boarded floor of a closet, under the roof of a large room; the weather was hot and dry, and from being just under the roof, they were exposed to the action of the sun. Two were examined on the sixth day, and two on the seventh day, after death. In all, putrefaction had very far advanced; indeed, they were perfectly putrid and horridly offensive. The contents of the stomachs were collected and distilled; the stomachs were also cut up and placed with distilled water in another retort. There was not the least trace of acid in the liquid from either retort.

Examination of Five Dogs Forty-three Hours after Death.

To two of them, iron had been given; one had been placed under the douche, one had had the acid by the mouth, having on the previous day had it by the vagina, but not sufficient to destroy it, no antidote or remedy having on either occasion been used; and the other had been destroyed by having had the acid introduced into the rectum. The quantity of acid which had been given was, in two, ten minims; in one, five minims; a smaller dog than either of the two, in two, having had before death seventeen minims of acid, viz. twelve minims by the mouth, and the day before about five by the vagina, also less than these two, but larger than No. 37; and No. 22, six minims by the anus, of the same size as No. 40, but not so old a dog. The smell was beginning to be offensive, though all were very stiff and rigid. The muscles were natural in colour and appearance. In Nos. 51 and 52, where the iron had been given, there was the greatest change; the least in Nos. 22 and 37; indeed, in these, especially No. 37, there could hardly be said to be any disagreeable smell.

In all, the liver was soft; there was no alteration in the gall-bladder; the spleen was natural; the stomach much contracted, and the mucous membrane red; the lungs were not engorged, but appeared *shrunken and contracted;* heart distended on both sides; blood in it equally dark in all the cavities; in one dog, fluid; in the others, loosely coagulated. The stomachs of Nos. 51 and 52 contained plenty of iron; the stomachs of these two were cut up, washed with distilled water, and with the contents distilled in separate retorts. There was not the least trace of hydrocyanic acid, neither was there among the contents any appearance of Prussian blue. The stomachs of Nos. 12 and 37 were also cut up, washed, and, with their contents, distilled. The distilled fluid yielded a very faint odour of hydrocyanic acid; and the nitrate of silver, as well as sulphate of iron tests, showed slight, but still decided traces of it.

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* Edinburgh Med. and Surg. Journal, Vol. li. p. 39. Edinburgh, 1839.

As it appeared desirable to ascertain the condition of the head and spinal marrow at or soon after death, I made three examinations.

In one the blood was fluid and dark-coloured; the muscles were extremely rigid; there was no unusual injection of the brain, or of the spinal chord and its membranes. In other two the brain was natural, not injected. In one a little clear serum was observed upon the surface, and within the ventricles. The sinuses were in one quite full of dark-coloured blood. In the same, in which death had been delayed after the first dose, the *pia mater* of the brain and the theca of the spinal marrow were congested; the vertebral sinuses also were full of dark-coloured fluid blood, not the medulla, and scarcely the *pia mater* of the chord.

In all the dogs that I examined the stomach was found to be very much contracted, the mucous membrane excessively corrugated, and of a brownish reddish colour.

The usual appearances were the following, varied according to the mode in which death takes place:—

A little serum was found within the cavities of the pericardium and pleuræ, apparently beginning to be effused. The left ventricle was firmly contracted; the right ventricle filled, but not distended, with fluid blood. The two auricles and the coronary veins, the *venæ cavæ*, and the pulmonary arteries and veins, all distended, the first excessively, with blood. The aorta and its branches were in several cases empty. The two auricles and right ventricle contracted in one case rapidly, but apparently without the power to expel their contents. The lungs were red in colour from contained blood. The blood was uniform in colour, darker than natural, and muddy in appearance.

In one case, a kitten, it was not until at least six hours had elapsed after the whole cerebro-spinal axis had been taken away, and more than six hours and a half after it had breathed, that all the cavities of the heart ceased to move. This they did in the usual order, the left ventricle soon being empty, contracted, and quiescent, the right auricle continuing to move the longest.

Though the heart in pulsating does not appear to propel the blood, yet the cavities gradually become less, the blood probably being forced out by the tonic rigidity which supervenes; so that, although its motions become more feeble, until some extraneous stimulus, as a sharp prick, is required to excite them, by the time they have altogether ceased the heart is generally empty, very small in size, and hard from its contracted state. This is a condition which I have observed in nearly every animal examined while the heart has been acting.

Blood.—The condition of the blood would naturally excite considerable attention. An attentive examination of it, however, does not yield any very positive results. The appearance of the blood to the naked eye is often materially changed, not only in colour, but it looks muddy and broken down, while its important property of coagulation is so interfered with, that it seems to be the generally received opinion, that after death from hydrocyanic acid the blood is almost invariably found in a fluid state. Yet this is by no means the case; and unless other observers shall be more successful than myself, the examination of the blood will throw but very little light upon the subject. If, however, any inference is to be drawn, it will tend to confirm the opinion that the action of the acid is upon the central nervous mass, the blood being only secondarily involved. Though the blood to the naked eye is often materially changed in appearance, yet when examined under the microscope, no decided difference is seen between the globules of blood drawn from an animal before the administration of the acid and that taken immediately after death has ensued from it, whether the acid has been given by the stomach, or through the lungs by inhalation. Experiment 141 *bis* shows that even the almost daily saturation of the system for the space of fourteen days with the acid does not change the character of the globules; and that the acid has, in reality, no peculiar action upon the globules is shown by the fact that, when it is added to healthy blood just drawn, on examination it is found not to produce so great an effect as a similar quantity of water does; indeed, the globules remain tolerably perfect and in motion for a considerable period, the appearance of the clear central nucleus gradually becoming more distinct. It might, therefore, be supposed that the action of the acid was limited to the fibrine; yet it is extremely doubtful if there be any decided change in even the fibrine; for the foregoing observations by no means confirm the idea of the fluidity of the blood after death from hydrocyanic acid; on the contrary, in by far the great majority of cases the blood is found to coagulate, though by no means invariably so; possibly coagulation is somewhat delayed, and certainly in many instances the coagulum is found less dense than in blood abstracted from the body. But it must be borne in mind that after death from almost any cause, the coagulation of the blood in the vessels is hardly ever complete, and often very imperfect, as compared with that which possibly has been abstracted from the same body only a few hours before death. In order more certainly to test the effect of the acid upon the fibrine, as the blood was flowing from the arm of a woman, bled on account of an inflamed leg, into one cup I dropped six minims of acid while the blood was flowing. The only difference observed between this cup of blood and a corresponding one without the acid, was, that it was thought the blood in the cup which contained the acid coagulated somewhat more slowly, but that the coagulation after a short time was more complete, and the clot more dense, than in the other.

So, also, though the colour of the blood is very frequently darker than natural, it is not always, as several of the experiments show. Thus in No. 112, where out of four toads which were destroyed in a similar manner, in two the blood was florid, while in only one was it very dark; and in cats, Nos. 89 and 98, the blood was of the brightest, most florid, hue I ever saw any arterial blood; and the blood which flowed from the jugular vein of the dogs, experiments 80 and 81, was certainly not darker than usual.

These experiments sufficiently prove that in whatever way hydrocyanic acid acts, it affects the whole animal kingdom (if not the vegetable also) in a similar manner. Some animals, it is true, are more obnoxious to its influence than others, but even in this respect the difference is not nearly so great as has been supposed. Those who have imagined that reptiles, amphibia, and the cold-blooded animals generally, enjoy an almost perfect immunity from its deadly properties, have been deceived. True it is, that these creatures do require a larger quantity than warm-blooded animals to affect them; but the difference in the time required for the development of the symptoms is far greater than that of the quantity, it being often several minutes before any symptom whatever is manifested. Experiment 107 will show that considerable power may be retained for a long period; and in one instance, I saw a frog, which subsequently died, leap a foot in length, at least half an hour after the acid had been given. Whether the larger quantity which may be taken, and the longer time required for its manifestation, bears any ratio to the slowness of the circulation, or the different condition of the nervous system, it is difficult to say positively. Those who suppose the acid to act by being absorbed into the blood, would incline to the former opinion, and, perhaps, consider the question proved by the well established fact of the absorption of the acid; for not only may its presence in the blood be detected chemically, but it is exhaled from the lungs in respiration within a short time after it has been swallowed, or even, as in experiment 22, after it has been placed within the rectum. Those, again, who lean to the supposition of the difference being dependent upon the condition of the cerebro-spinal axis and the nerves, would point to the curious fact of the acid, whether it be introduced into the blood by absorption from within the living body, or merely by adding to it when abstracted, producing no greater effect, if so great, in its physiological properties, than a similar quantity of water does; and also to the fact of some warm-blooded animals, where there is no corresponding difference in the condition of the circulation, being more obnoxious to the poison than others; thus the rabbit and the mouse (perhaps all rodents) are more susceptible to its action than is the cat, and the young animal of the same species than the old, while in these the tenacity of life and the powers of the nervous system are more equable and enduring than in the young.

When the effects produced by a dose of hydrocyanic acid are not too rapid to be observed, the earliest appear to be giddiness, with a loss of muscular power and control over the combined action of the muscles; the head droops, the mouth is open, the tongue protrudes, there is apparently a sense of distress and constriction in the fauces, and the respiration is hurried and panting; but I doubt whether this condition of breathing is not dependent upon the state of the muscles. It would seem they are unable to expand the chest deeply, gradually, and efficiently. The muscles of the pharynx and larynx are very soon affected, and, indeed, it might almost be supposed that these are peculiarly susceptible to the action of hydrocyanic acid. There are many persons who are unable to detect the presence of the acid by the sense of smell, yet who are at once cognizant of it by a peculiar feeling of dryness, and the sensation of constriction, in the throat; and even those persons who detect it readily enough by the nose are also conscious of this feeling. When any one has been exposed for some time to the vapour of the acid, even when not very concentrated, this feeling in the throat continues for some hours. Thus in the case of Mr A———, which is reported in the *Provincial Medical and Surgical Journal* for July 23d and 30th 1845,* with whom I remained upwards of half an hour before he died, and shortly afterwards made an examination of the body, I did not altogether lose the feeling until the next day. The true explanation, however, of this is probably, not that these parts are really more susceptible to the action of the acid than the others, but that in the act of respiration it is brought immediately into contact with them.

This unsteadiness and loss of power is speedily succeeded by spasm,—frequently of the most intense kind. This may be either tonic or clonic, according to the strength of the dose of acid and the susceptibility of the animal. The stronger the dose, usually the more violent and continuous is the spasm. If the spasm be tonic, not only do the voluntary muscles continue rigid, but the heart is, at least, to the same extent affected. It ceases to pulsate, the diastolic action is suspended, and consequently the circulation is at once arrested, while the respiration is also equally, and at the same instant, prevented by spasm of all the muscles by which the function is performed. Hence, the lividity and bloatedness of the face; the protrusion of the eyes; and the excessive congestion of the veins. In this condition of these two all-important functions is to be seen one of the causes of the almost instantaneous death which takes place in some instances.

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* Of this case, and one in which recovery took place, an abstract is given in the Medical Intelligence.

When, however, the dose of the acid is less, or the susceptibility of the animal not so great, the rigidity of the muscles soon abates and gives way, and is followed by paralysis more or less complete; when the respiration either becomes excessively rapid, the chest being but little expanded, or it is very slowly performed, being then deep and sighing. The heart now beats most rapidly and feebly.

This state of paralysis may continue, the weakness gradually increasing until the animal dies, or, as more generally occurs, the creature is convulsed and paralyzed at intervals, the heart continuing to beat very feebly for two or even three minutes after all indication of sensation and other motion have ceased; though by no means unfrequently a strong convulsion immediately precedes death. In some instances I have observed respiration continue quite as long as the heart could be felt to pulsate. If the animal recovers, it will often lie in the most complete state of paralysis possible; after a time some little convulsive movement in the limbs, or some spasmodic action in respiration, may be observed. These gradually become stronger, until the convulsions often become very powerful. It now howls loudly, reflex action returns, and, after a time, sensation. The utter powerlessness of the creature gradually becomes less, and sensibility is evinced. It will now sit or lie as though in a comfortable sleep, and, after a varying period, will either wake up and appear surprised and walk slowly away, or it will continue very feeble, but little disposed to move, and with very little command over its muscles.

I do not agree in the opinion which has been expressed by some, that the heart only ceases to beat because of the suspension of respiration, and its consequently containing only dark blood. It rather appears to me that the cause of death is altogether different from what occurs in asphyxia from occlusion of the air passages, the heart being primarily, and, to at least an equal extent, affected as respiration. This is not merely a theoretical question, but a most important practical one, as it must materially influence the treatment. If the heart's action only ceases on account of the want of respiration, and the consequent engorgement of its cavities with dark blood, venesection would be one of the remedies to which we should first have recourse, and from which we should expect the most decided benefit; while, on the contrary, if the state of things be as I suppose,—not only that the blood itself is changed by the introduction of the poison, but that the heart is primarily affected, our reasons for having recourse to venesection would be much less powerful; neither should we expect the same amount of benefit from its employment. The results of experiments 79, 80, and 81 do not justify the expectation of any very marked benefit from the abstraction of blood; for, though there is experiment 54, in which the dog recovered after having had an emetic and being bled, I do not attribute this to the abstraction of blood, but rather to the small dose of acid in proportion to the size, age, and breed of the dog; I am inclined to think the result would have been the same had the blood not been taken. Besides this, it was an artery which I opened; this would, of course, relieve the left side of the heart, and consequently could have little influence in inducing a continuance of its action, according to the theory of those who think the heart only ceases to act on account of the distension of its cavities from impediment to the passage of the blood through the lungs. If this were so, it would be the right side of the heart, not the left, which would principally be engorged, and which should be relieved by venesection. If the bleeding in this case were really useful, it would tend to support the view I have above taken of the condition of the heart.

It must not be understood that I am arguing against the abstraction of blood in every case, or denying that congestion does occur. Under all circumstances, when the respiration and circulation are feeble, and more especially in similar cases to those under consideration, where there is also spasm, congestion is apt to take place. Venesection may, therefore, in some few cases, where the dose of acid has been small, be proper and advisable enough, when practised with the understanding of why we employ it, and too much blood be not abstracted; but as there is great loss of power as well as spasm at the time, and where recovery takes place considerable weakness and depression are for a while manifested, the taking away a large quantity of blood, in my opinion, is likely to be rather injurious than beneficial. The condition of the heart, at least at first, is one of contraction, not of dilatation. There is a persistance of contraction, not a diminution of contractibility, so long as the spasm continues, and of the left cavities for long after. The cavities, it is true, may possibly occasionally afterwards become engorged, but this is only when the heart, like the general muscular system, becomes paralyzed, and certainly is not supported by the condition of things as revealed by the post-mortem examinations above recorded.

Though the sensorium is soon affected, as shown by the vertigo, which is one of the earliest symptoms, it is not that portion of the cerebro-spinal axis which appears to be most affected by hydrocyanic acid. At the time I was with Mr A———, I could not divest myself of the feeling that he retained some degree of consciousness, and was to some extent cognizant, at least in part, of what was going on about him (and even at first, when I shouted in his ear, that he recognised my voice), at a time when he was unable to give much indication of sensibility. From what I have since seen in many of the animals I have experimented upon, I have little doubt they retain some consciousness and sensibility when, from the powerlessness of their muscles, they are unable to indicate it. Of course when the powerlessness is complete, it cannot be proved that either, in any feeble degree, is retained; but it may fairly be inferred from what is witnessed in such cases as Nos. 11, 23, 28, 33, 36, 39, 88, 90, 91, and others, that the acid acts more upon the nerves of motion than upon those of sensation. To what extent consciousness is retained it is difficult to say; I am, however, inclined to think it is preserved to a longer period than many are even now disposed to allow. The many cases which have been reported where parties have swallowed large doses of hydrocyanic acid, and yet retained consciousness for some time, prove that it may be retained longer than was formerly supposed to be possible; but, even in these, consciousness is thought to be retained only so long as the power of voluntary motion continues. It, however, appears probable in some cases, where either the dose has not been sufficiently large to prove suddenly fatal, or the susceptibility of the animal to its action but little, that some degree of consciousness may be retained, after all power of voluntary motion is lost, and even when there is but little sensibility remaining; at least, some of the foregoing experiments seem to indicate this. If this be correct, it would prove that the sensorium is not that part of the nervous system which is most affected by hydrocyanic acid, so also the violent spasmodic action, occurring alternately with paralysis, independent of volition, would point rather to the spinal marrow than to the brain as being most obnoxious to the action of the poison.

Whatever may be thought of this opinion, there are few persons, I apprehend, who will be inclined to dispute, that upon one most important practical question, these experiments are quite decisive, and fully confirm the reports which have been made of the length of time which not only consciousness, but perfect volition and entire control over voluntary motion, may be retained after a dose of hydrocyanic acid, sufficiently large to prove fatal within a few minutes, has been taken. In some few instances, as Nos. 8, 9, 15, 18, 27, and 36, the action of the poison was so expeditious as to prevent almost the slightest exhibition of voluntary motion; but in the majority of dogs and other warm-blooded animals, about twenty seconds elapsed before any symptoms were manifested; and this space of time in a man would allow of several actions being done, as the corking of a bottle and placing it in the socket, or upon a shelf, the possibility of which has been much doubted. In several of the dogs, a much longer interval intervened, during which they gave no indication of having had hydrocyanic acid. Thus the time which elapsed before any decided symptoms of poisoning were shown in Nos. 4, 6, 13, 14, 70, was fully sufficient to have permitted a man to have performed many voluntary actions, and to have not only put away anything which would have revealed the nature of what he had taken, or how he had taken it, but also to have passed to a considerable distance from the place where he had swallowed the poison; while a still more remarkable retention of consciousness and power was seen in 24, which went down three or four steps of some stairs, saw the door at the bottom was closed, and came back again; in 56, which went down, came up, and then went down again, the whole flight a steep winding staircase; and in 38, which retained sufficient vigour to jump over one of the dogs, and then actually leaped completely across the open top of the staircase, In No. 22, to which the acid was given by the rectum, fully two minutes elapsed before symptoms set in, and it was not until a further period that voluntary motion was lost, yet the animal died in thirty-seven minutes; while in a few, but only a few instances, as No. 60, where the effect was almost immediate, yet the dose was not sufficient to destroy life.

Reflex action is that which is last destroyed and first returns. This continues for some time after all voluntary motion and sensation have ceased. Touching the cornea will induce winking of the eye-lids when there is nothing to indicate the retention of volition or sensation, and irritating the nostrils of a frog will cause depression of the eye.

When the dose of acid has been sufficient speedily to destroy life, none but an ignorant or careless person would confound the effects with those produced by opium; but if the animal survives for some time, it is very different. The deep, quiet sleep, with the great indisposition to be roused from it, so closely resemble those from opium, while the condition of the pupil is so variable, being often in this stage by no means dilated, that not only is there great liability for the effects produced by the acid to be confounded with those of opium; but I have very strong suspicions that in at least one recent case where death was supposed to have been induced by opium, it, in reality, was caused by hydrocyanic acid. Experiments 56, 88, 90, 91, and 141 *bis*, will show the symptoms which most resemble those seen after opium.

The acid acts locally as well as generally. This is seen not merely by its producing vascular congestion, but also by the decided effects upon the nerves of the part. Thus when it is dropped in one eye of an animal, the pupil of the eye is sooner, and to a greater extent, dilated than that of the other; and when the acid is administered by the rectum or the vagina, both hind legs are sooner affected than the anterior portion of the body. But that the acid does not directly paralyze muscular fibres, when locally applied, is shown in the experiment upon the frog, No. 108, where a drop of acid, applied upon the heart itself, acted as a stimulant, inducing a quickened action; possibly any other fluid would have done the same; had, however, the acid been a direct sedative, such an effect would not have continued for so long a time, and perhaps more decidedly still in the cat, No. 96, where three drops were put upon the heart without arresting its motion.

Necroscopic examinations, whether made immediately after life has ceased, or not until some time afterwards, do not throw any very decided light upon the cause of death; indeed, until I had examined the state of the heart immediately the acid had produced insensibility, I had, like other observers, considered the evidence as altogether of a negative character; for the reports of the appearances found in examinations made some time after death of the bodies of men and animals who had died from the effects of hydrocyanic acid are too contradictory to be at all conclusive. Such, also, I found to be the case with the animals I examined a day or two after death; but if the body be opened while the heart is pulsating, this contradiction in the reported condition of the heart and great vessels is in a great degree explained. If death has been long delayed, or the dose of acid very small, the blood is usually dark in colour, alike on both sides of the heart, and all the cavities of the viscus may contain more or less blood, especially the right, which is often much distended; while, on the contrary, if the death has been sudden, in almost every case the left side of the heart, and especially the ventricle, is found to be perfectly empty and rigidly contracted, while the right side of the heart contains blood, being in some cases, though by no means always, much distended.

That this different condition of the two sides of the heart does not depend merely upon obstruction to the flow of the blood through the lungs is shown by many of the post-mortem examinations, but I would refer particularly to those of the two rabbits and two kittens, Nos. 86 and 87, where not only the right side of the heart, *venæ cavæ*, and the pulmonary arteries were filled, but also the pulmonary veins and left auricle, while the left ventricle and aorta were contracted and empty. This, when taken in conjunction with the appearances found in the two kittens so suddenly destroyed and immediately opened, Nos. 142 and 143, where not only the left ventricle was empty and the auricle on the same side also, but the lungs were white and collapsed, their blood vessels not filled, and the right cavities by no means distended with blood, proves, beyond a doubt, that this condition of the heart is dependent upon causes acting upon itself, and that it does not merely depend upon any change in the respiratory function.*

In nearly every instance the aorta and its large branches were, like the left ventricle, found empty. That the large arteries are, after death from most causes, found to contain but little blood I am aware, but I do not think there is ordinarily such a bloodless and contracted condition. It would seem as though not only the left ventricle propelled the whole of its blood, and continued in persistent contraction, but that if the contractile power of the great arteries be not absolutely increased, by the diminution of opposing force, it is relatively so.

I am not, however, convinced (but, on the contrary, rather incline to the opinion) that if the contractile power of the involuntary muscles be not increased, at least the persistence of it may yet be. The movements of the alimentary canal, throughout its entire extent, are not only most decided, but long continued, and afford by far the best opportunities I have had of observing the peristaltic action of the intestines, and the important use of the mesentery in preventing entanglement and intussusception of the gut. So strong are the motions of the different portions of intestine, that the attached mesentery is put fully upon the stretch. The passing of urine and fæces so often witnessed during the action of the poison, would not of itself be regarded as of much importance in this argument, as it might arise from paralysis of the sphincters; but when it is considered that it usually takes place during the time of rigidity, and that not unfrequently there is considerable straining to pass both fæces and urine, with protrusion of the rectum, and, at times, of the penis, in connection with other circumstances, it is worth bearing in mind.

The curious fact of the independent action of the different cavities, and even of different portions of the same cavity, of the heart, as well as the curious order in which their contractibility disappears, the right auricle apparently possessing the greatest persistence of action, so frequently alluded to in the post-mortem examinations, and especially the property which experiment 88 and others, upon the rabbit and kittens, fully prove the heart to possess of continuing for long to contract after the brain and spinal marrow are entirely removed, even in warm-blooded animals, also supports this view. I would venture here merely to refer the reader to the detail of these experiments. The facts could not be passed over in this account of the action of hydrocyanic acid without being alluded to; but to follow them out would lead us too far away from the subject immediately under consideration, and their physiological bearing is far too important not to demand a full investigation, the result of which I must elsewhere relate. I may, however, state, that while I do not consider them as proving the truth of the Hallerian hypothesis of the independence of muscular contractility of the brain and spinal marrow (for the determination of which a far more extended series of experiments must be made), they are, at least to my mind, fully sufficient to disprove the opinion of Flourens as to the dependence of the heart's action solely upon respiration, inasmuch as in these experiments, and many others, the whole anterior thoracic parietes had been removed, and, consequently, no respiratory movement could take place. They not only appear decisive as to the incorrectness of the statements of Le Gallois; for the entire mass of the brain and spinal marrow was removed; but they also strongly tend to show, that whatever countenance may be given by them to the conclusions of Wilson Phillip, they at any rate prove that the sudden taking away of the brain and spinal marrow does not, as he supposed, at least under all the circumstances, arrest the action of the heart. And further, if it be admitted that these parts are under the dominion of the sympathetic nerve, not possessing an independent inherent power of contractility, the experiments would, at any rate, prove, first, that the office of the sympathetic nerve is not merely to bring those parts to which it is distributed under the influence of the motor power of the cerebro-spinal axis, but that it in reality is an independent nervous system, being only connected with the spinal marrow, and not derived from it; secondly, that as its functions are different, so is it differently affected by the same means; for while the power of the cerebro-spinal system is utterly destroyed by the action of hydrocyanic acid, that of the sympathetic is scarcely or not all affected by it.

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* This condition of the heart and great vessels explains the reason why, with the venous system so much congested, so little blood flows from a vein when opened. The circulation is in truth arrested. Though the heart moves, it does not propel the blood.

An appearance which I have almost invariably found present is the congested condition of the membrane upon which the acid is applied. Thus the stomach is found when empty to be much contracted, and the mucous membrane of a brick-red colour. The same appearance of congestion is seen in the vagina, the rectum, and conjunctiva, when death has followed the application of the acid to these parts. I have thought the lungs were found to present a collapsed condition when the poison has been swallowed, but when inhaled the lungs are frequently, though not invariably, much congested, scarlet in colour, and in an emphysematous condition from distension of the air-cells.

The examinations will show that we may in vain look for any decided change in the substance of the brain and spinal marrow. At first, when the examinations had been limited to dogs, and those principally which had been dead some short time, I was inclined to think the little congestion of their membranes was a *post-mortem* effect, depending upon the position of the animal and the fluidity of the blood. I had not then observed that in nearly every instance, the vertebral sinuses are remarkably engorged. On finding this in almost every case, even on immediately opening the canal, for some time I thought this was to be regarded as something positive and explaining, or at least connected with the state of the spinal marrow and the excessive convulsions, but more numerous examinations lead me to doubt if the distended condition of the vertebral sinuses arises from anything more than the general distension of the veins throughout most of the body, for I have observed the two exist in the same degree in the same cases. If the convulsion have been tonic, the vertebral sinuses and veins generally are much distended; on the contrary, if the convulsion be less, there is less venous distension, though it is never altogether absent.

It at first seems singular that the vertebral sinuses should be almost uniformly so much engorged, while the cerebral are not; but this, perhaps, admits of explanation, by the fact of the vertebral sinuses being rather the receptacle for the blood which has circulated in the surrounding parts, than in the medulla itself, while, with the cerebral sinuses, it is just the reverse. Hence, probably, the circulation in the spinal marrow is in reality not more disturbed than it is in the brain, for if it were, we should find the congestion in the veins accompanying the spinal arteries, instead of in the vertebral sinuses, which we do not. We must, therefore, conclude that the symptoms exhibited depend upon some functional perversion in the action of the nervous system, or upon some physical change, which either disappears with life, or is of so slight and subtle a character, as hitherto to have escaped detection, being confined to the more minute organization; or, on the other hand, we must consider the persistent contraction of the left ventricle of the heart as a primary effect of the acid, the suspension and perversion of the cerebral and spinal functions being secondary and dependent on the want of a due supply of arterial blood, and, possibly, on the congested condition of their veins.

In the case in the *Provincial Medical and Surgical Journal* before referred to, it is stated that the acid was easily detected in the contents of the stomach twenty-three days after death, although no pains had been taken to preserve it. This appeared to be in direct contradiction to the general opinion as to the speedy disappearance of the acid. From what I have since seen, I do not now doubt that the preservation of the acid for so long a period was owing to the contents of the stomach having been removed from the viscus, and to their being strongly imbued with gastric juice, the antiseptic properties of which were sufficient to preserve the whole mass from decomposition. The examination of the animals now reported does not confirm the supposition, that, under ordinary circumstances, hydrocyanic acid can be found many days after death, even when the body has not been disturbed. If decomposition is not accelerated, it is certainly not delayed; and when this has advanced to any extent, it is probable the acid will have participated in the change. I am inclined to think the rigidity after death from hydrocyanic acid is greater than after death from some other causes. I may also mention that animals poisoned by the acid may be eaten by others. The trout, killed by six minims of acid, was eaten the same night by a cat without any ill effects; so also were some of the rabbits, and others were blown by flies, the maggots of which feasted most luxuriantly upon them.

There is no fixed quantity of acid which will invariably destroy life. The boundary between the dose which is hazardous, or even will destroy life, and that which may be taken with impunity, is very slight and indefinite,—dependent upon individual, and, probably, varying circumstances. The same creature is, I apprehend, liable to be seriously affected by a dose which at another time would produce but little effect. This, indeed, appears to be fully proved by the varying effects produced upon the cat, No. 141 *bis*, by exactly the same doses of acid. The more vigorous the animal *cæteris paribus*, the larger is the quantity which may be taken. The stomach being full of food lessens the effect of the acid, probably by its mixing with the acid, and thus preventing the poison coming into such ready contact with the mucous membrane. At this period there is also more vigour in the system, whereas an empty stomach allows the immediate contact of all the acid at once, and a hungry animal possesses less power of resistance. It was owing to the stomach being nearly filled with food recently taken at breakfast, that I think may fairly, at least in part, be attributed the long continuance of life after the swallowing of so large a quantity of hydrocyanic acid as there is every reason to suppose was taken by the person whose case is reported in the *Provincial Medical and Surgical Journal* for July 23, 1845. The age of the animal, I am quite satisfied, makes a material difference. The foregoing experiments, I think, fully prove that a much smaller dose of the acid is fatal in young animals than in old; and that almost in direct proportion to the youth of the creature is it speedily and fatally brought under the influence of a smaller dose of the acid. Several of the experiments would, however, show, as Nos. 87,* 87,** 90, and 91, that this must be taken with some limitation, for it would appear that the *very* young animal is actually less susceptible, and requires a larger dose of the acid to destroy it than does one of the same species a little older. This is so curious a fact, that, were there not sufficient evidence to support it, we should feel much inclined to doubt it. Is it to be regarded as another proof of the approximation of the young of the higher species to the adult of the lower?

The degree of concentration of the acid has no very material influence over its action. The dogs to whom a diluted acid was given were quite as speedily and violently affected as those to whom acid of Scheele's strength was administered. Indeed, I am inclined to think, from observing the action of the concentrated acid, containing twenty-five per cent. of real acid, that a moderate degree of dilution renders the action of the acid more speedy, probably from bringing it at the same instant into contact with a larger surface. Certainly the foregoing experiments show that dilution to a considerable extent does not weaken the action, if it does not rather accelerate it. The contrast between experiments 11 and 35 would by many seem to prove greater effect and rapidity in the very dilute acid; it must, however, be borne in mind that No. 11 was an adult dog, while No. 35 was a young one.

The action of the acid is not proportionably speedy to the quantity taken. The difference between the rapidity in the effects of a large dose of the acid and of one which is so small as to be barely sufficient to destroy life, is certainly well marked; but this is not the case between two doses, each of which is capable of destroying life with moderate rapidity; as, for instance, suppose forty minims of Scheele's acid will kill a dog within four minutes, it does not follow that eighty minims or two drachms would do so in two minutes, or in one. Hence, when called to a person poisoned by hydrocyanic acid, we cannot, merely from the length of time he has survived, or the violence of the symptoms, determine anything with certainty, as to the degree of concentration or dilution of the acid, nor, except within wide limits, much as to the absolute quantity taken.

In determining whether hydrocyanic acid possesses an accumulative power or not, it is necessary first to determine what we understand by this. The foregoing experiments clearly show that where one dose has not been sufficient to destroy life, a second smaller one, which by itself would not have been sufficient to kill, if given before the effects of the first have passed away, immediately induces more violent symptoms, and that death speedily ensues. Thus, within certain limits, hydrocyanic acid may be said to be accumulative; but in this sense almost every medicinal preparation is the same. If, however, it be meant by those who imagine that the acid is accumulative, that doses of it given on different days may be so small, as singly to produce no injurious effects, and might otherwise be continued with impunity for an indefinite period, yet that these distinct quantities may remain in the system unchanged until the whole aggregate quantity shall suddenly produce the effect of a larger quantity given at once, I must *in toto* dissent from the opinion. Indeed I think we may fairly consider the result of experiment 141 *bis* fully sufficient to prove that hydrocyanic acid does not possess this power. On the contrary, many would, and I believe correctly, regard it as proving the contrary,—that the system becomes habituated to it, and by repetition will endure a dose which, if given at first, would have been destructive.

Whether or not hydrocyanic acid be decomposed in the system I am not prepared to say; but it is certain that it is readily absorbed, and also that it passes off with the respired air by the lungs; for the breath of poisoned animals, both warm and cold blooded, strongly exhales the odour in a short space of time, and I have no doubt the whole is quickly passed off. It does not, however, necessarily follow that the effects of it as speedily pass away; indeed, several of these experiments show that they do not, particularly experiments 39, 56, and 141 *bis*. It is the same with alcoholic drinks, yet no one attributes to them an accumulative property. Hence, a dose which may with impunity be given two or three times a day, because there is time not only for the acid to be given off, but for the effects upon the system to subside, cannot, I think, prudently be repeated every hour or so; for though it is possible the acid may have been given off, I do not think the system would have had time to recover from the depressing influences of it. It was curious to observe that however helpless and paralyzed a dog might be lying, if another dose were given, the first and *immediate* effect of it was to rouse it, and often to produce violent motion.

The poison acts with almost, if not absolutely, equal rapidity and certainty when applied upon a mucous membrane, as the conjunctiva, the rectum, or the vagina, as when swallowed. (See experiments 20, 21, 22, 23, 24, 25, 98, and others.) A knowledge of this fact may be of the utmost importance in a medicolegal examination, as it would not be difficult either by force or cunning to introduce into the vagina, or rectum, or put upon the eye, a sufficient quantity of the acid to quickly destroy life—a mode of administering the poison which a murderer who had sufficient acquaintance with its properties is not unlikely hereafter to adopt. Experiments 86, 87, 88, 89, 101, 103, 112, and others, will prove that the action of the acid upon the lungs when air impregnated with it is breathed, is not only rapid but certain in its effects, and forms one of the easiest methods of exhibiting it,—one which it would be very easy to employ, but most difficult after a few hours to detect, as the odour, being so diffusible, is very soon dissipated.

It is probable that the acid applied to an ulcerated surface would speedily induce death; but of this I have hitherto not had any opportunity of ascertaining the truth by experiment. It would, however, appear, if we may form an opinion from a single experiment, that this is not the case with a recent wound.* I did not repeat the experiment, being averse to inflict the wound for the purpose, as there is not much probability, even if the acid would act freely when so applied, of it being thus used with a criminal intention. I do not think there is reason to suppose the acid readily acts through the epidermis. I have put it within the external auditory meatus, and upon the surface of the body, with little effect. If applied largely it might penetrate, and thus act, or its evaporation will act through the respiration.

Much has been said on some recent trials as to the death shriek, as it was then termed, in persons who are suffering under a poisonous dose of hydrocyanic acid. Though it is now generally admitted that the shriek does not uniformly occur in man, it does not appear to be so generally known that this is also the case with animals. The foregoing experiments will, however, show that so far from the shrieking being characteristic of death by hydrocyanic acid in dogs (and I may also add in other animals, as rabbits, mice, cats), it occurs in only half the number of cases, and in not more than one-third, if quite so many, very loudly; but when it does occur, the cry is of so peculiar a kind, and so indicative of severe distress, as to give the idea of consciousness on the part of the animal of impending death,—as though it felt that its condition was such as to render all assistance unavailing: it is different from anything I have heard in any other condition of dogs or other creatures, and is, I think, when present, characteristic of the poison.

In, perhaps, a tenth of the cases the fæces alone are passed; in another tenth, both fæces and urine; and in a far larger number, the urine alone: but it so frequently happens, probably as often as in two-fifths of the whole number of cases, that neither fæces nor urine are passed, that we cannot rely much upon these as symptoms. In neither of the two cases I have seen in man were either passed. Their expulsion appears to be owing to the violent spasmodic action of the voluntary muscles assisting the contraction of the involuntary, as the rectum is usually unloaded and the water passed during a strong rigor,—often during the violent one, which not uncommonly immediately precedes death. If, instead of a mere unloading of the rectum, there is purging or tenesmus, it may be regarded as a favourable sign, as it was observed in nearly all the dogs, and in several of the cats, that recovered. Vomiting is, of all the symptoms, the most favourable, not only, I apprehend, from the unloading of the stomach, getting rid of part of the poison, but also as indicating that the system is not fully under the influence of it. Vomiting does not occur in recovery after inhalation.

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* This is opposed by the statement of Sobernheim, of the apothecary who died from the application of the acid to a recent wound of the hand, occasioned by the breaking of a glass vessel in which the acid was contained. It is, however, probable the quantity of acid was large, and not at all unlikely the effect might in reality have been produced by the acid being diffused through the air, and thus acting upon the lungs by respiration, a mode by which, as the foregoing experiments fully prove, hydrocyanic acid acts with considerable rapidity and power.

I know of no antidote to hydrocyanic acid.

Chlorine has been much spoken of by many persons, but it is probable these parties have advanced the claims of chlorine rather on theoretical and chemical grounds than from any experience of its utility. It will be seen from experiments 56, 57, and 58, that little good is likely to result from it. In these dogs it was administered much earlier than it is ever likely to be in the human subject, and yet they died quite as soon as those dogs to whom a like dose of the acid had been given, and where nothing was done. We should, therefore, I think, not be justified in placing much reliance upon its efficacy as an antidote. Some persons may consider the quantity of chlorine given not sufficient, but the extreme difficulty, often impossibility, of getting anything swallowed after hydrocyanic acid has been taken, must be borne in mind, and I apprehend chlorine can only be given in solution, and not as a gas by inhalation.

It appeared not improbable, that if an insoluble cyanide of silver could be formed, by administering a solution of the nitrate soon after the acid had been taken, good might possibly result; but this is fallacious. It will be seen from experiments 42, 43, 44, and 45, that the cyanide of silver itself acts as a poison, in a weaker degree, it is true, than hydrocyanic acid, but in a similar manner; and the termination in those dogs, and in the cat, to whom a solution of nitrate of silver was given immediately after the acid had been administered, by no means holds out much hope of any great advantage from this preparation.

Alkalies have been suggested, as readily combining with the acid, it being thought the compound would be less deadly than the acid. There appears not much value in the suggestion, and experiment 49 shows that potass is not likely to be of any avail.

The preparations of iron I thought worthy of a trial. It seemed not impossible that we might, if these were given sufficiently early, so far convert the acid into an inert prussiate of iron as to render it innocuous. Accordingly I gave iron in different forms,—the red oxide, the fresh precipitated green oxide, and the sulphate, alone and in combination with alkalies, but in all equally without such effect as to lead us to expect any advantage as likely to result from administering them as antidotes.

If the class of antidotes give us little hope of counteracting the poison by changing its composition, the expectation of relief from remedies against its effects is not much more cheering.

I had formed some anticipation of relief from electricity or galvanism, but the result of experiments 26, 27, 28, 29, and 30, appears to destroy every expectation of benefit from its employment. In experiment 28 the dog recovered after the first dose of acid, but this I believe it would have done had nothing been tried, the dose being small in proportion to the size and strength of the creature. It is true some muscular action is continued longer when galvanism is used than when nothing is done. This, however, appears not to be anything more than the motion which may be induced by galvanism in the limbs of any recently dead animal. Indeed, on comparing experiments 26 and 29 it would appear as if the galvinism, by its continued excitance, sooner exhausted muscular irritability, or nervous energy, for it will be seen that though muscular action continued much longer in No. 26, where galvanism was applied, than in No. 29, where nothing was done, yet that at the end of twenty minutes, when action could no longer be excited in No. 26, it was freely excited in No. 29.

Experiments 33 to 41 show the effects of cold water. This remedy has been very much relied upon from its apparently good effect in some cases, but probably much more from the symptoms appearing to require some such rousing of the system as cold affusion produces. Though the effects of it in these experiments are not such as to confirm the very high expectation which some practitioners have held out of its value, nor to lead to the conclusion, that where a large dose of the acid has been taken, much good will be effected by the use of it; yet, I think, we may fairly consider that when the dose has been of barely sufficient strength to destroy life, cold affusion possesses some power for good, and hence it is one of the means to which we should, without any loss of time, have recourse. In one or two of the cases, I am inclined to think, the plunging was rather injurious than not, and I must take the liberty of warning gentlemen against the supposition, that if the moderate dashing of water is useful, a more continued and general application of it would be still more so. This is most decidedly not the case. When the whole of the body was placed in water for more than a very short time, or the douche was repeated too often, or the water allowed to fall with too great a force, the dog appeared to suffer, and harm was done. A moderate quantity of water suddenly dashed a few times upon the face, or the head just plunged in water, produced better effects than placing the whole body in it. This produced depression and shivering. The contrast between experiments 56, 60, and 39, and 35 and 37 will show this. It must be borne in mind, that those dogs and cats which recovered were weak and depressed, and some were paralyzed for many hours, or even days, as Nos. 39, 56, 88, 90, 91, and 141 *bis*, and all appeared to feel the cold and to suffer from it, while warmth appeared to be very grateful to them. I would, therefore, venture to recommend that cold water should be merely dashed upon the head and face, and not, as some have advised, that the whole body should be put into cold water.

The effect of a good shaking ought not to be overlooked. In some of the animals the effects of the poison appeared to be delayed by this, and in several of those which recovered the return of sensibility and motion was, I thought, decidedly accelerated by it; indeed, from its effects upon animals, I am inclined to estimate its value as high as that of anything I have tried.

It is not necessary here to repeat what I have already said of bleeding. It has been stated, that in most of the dogs which recovered, vomiting took place. This also occurred in the man Jackson, who recovered after taking so considerable a dose of the acid. It might, therefore, be presumed, that the use of emetics would be beneficial. I tried the effect of three preparations,—sulphate of copper, sulphate of zinc, and tartarized antimony; experiments 59 to 71 will show the result. It will be observed, that if the dose of the acid were small, and vomiting were induced, the creature recovered; but, on the contrary, if large, the stomach became insensible to the action of the emetic, even though the dose of this were considerable, and administered immediately after the poison. We may therefore conclude, that where the quantity of acid swallowed has been so small as to be of doubful effect, or the condition of the stomach such, from the quantity of food which it contains, or perhaps spirits (Jackson had been drinking to some extent before taking the acid, and took this in gin; though we do not know what influence this condition of the stomach and system may exercise, the fact is worth bearing in mind), as to prevent or delay the speedy action of the poison; the administration of an emetic is proper, and ought to be practised, yet that the experiments hold out not the least hope of advantage from emetics where the quantity of acid has been considerable, even though immediately at hand to be instantly given.

But little difference in the action of the three different substances was observed, but of course it would be well to select that substance which acts most promptly.

Whatever may be the value of some stimulants, experiments 87, 87,* 87,** and will show that little advantage is likely to result from sulphuric ether. In these cases it was given in a way likely to test its properties in a more favourable manner than it is probable it ever could be in any ordinary case of poisoning by the acid; and yet, so far from any benefit resulting from it, if any effects were produced by it, the action of the acid was increased in power and rapidity.

It is, however, upon diffusible stimulants that great reliance is placed; and as of these ammonia is the one to which most value is attached, statements having been published which, if corroborated by experience, would show that at least one tolerably active remedy against the effects of the poison was in our possession, I was anxious to ascertain if those favourable opinions were founded in fact. Accordingly, a greater number of experiments with this stimulant were made than with any other supposed remedy or antidote; and they are so varied that it may not, perhaps, be thought presumptive to suppose they will enable us to estimate the true value which should be attached to ammonia. Experiments 73, 74, and 75 will show the effect of ammonia given internally within a very short time after hydrocyanic acid had been administered by the mouth; No. 100, the effect when given mixed with the acid; Nos. 76, 77, and 78, of it freely applied externally shortly after the acid had been swallowed; No. 97, of the effect of the vapour of it after the acid had been given by the mouth; No. 98, after the acid had been put within the eyelids; Nos. 88, 91, 94, and 99, illustrate the effects of the vapour of ammonia when mixed with the vapour of hydrocyanic acid, both being inhaled together; No. 95, of the vapour inhaled shortly after the inhalation of the vapour of hydrocyanic acid; and No. 96, of the vapour when inhaled previously to the giving of a dose of the acid by the mouth. This latter experiment was made in order to ascertain how far it was possible to prevent the development of the action of the poison by having the system well saturated with the supposed antidote. The *liquor ammoniæ* was as strong as possible, and in all cases the quantities given were larger than *cæteris paribus* are likely to be administered to man; we may, therefore, consider the test as fairly applied.

The conclusion to be drawn from these is, that though on the whole, under some circumstances, ammonia may possibly possess some virtue as a remedy, it by no means merits the praise which has been bestowed upon it, nor are we justified in placing much dependence upon it when the dose of acid has been at all considerable.

The external application (and it was freely applied), did not appear to be of the least value; nor was the administration of it by the mouth much more encouraging, either given shortly after the acid had been swallowed, or when mixed with it. The dog No. 75 rallied, it is true, after the ammonia had been given; but when the smallness of the dose of the poison, and the mode of its administration is considered, this case affords very little ground for attributing any other good effect to the ammonia than the inducing of vomiting, which cannot be regarded as the proper action of it, nor for the production of which it would be given. Neither was the vapour of ammonia, when used immediately after the system was affected from the internal administration or the external application of the acid, more successful, while the vapour of the acid appeared to be wholly uncontrolled by the presence of that of ammonia when this was added one minute after the animal had been subjected to it.

If those experiments, in which the vapour of both the acid and ammonia were inhaled at the same time, be compared with the corresponding experiments where only the vapour of the acid was inhaled, it must, I think, be admitted that though in some, as Nos. 88 and 89, the difference in the effects was too slight to be regarded; yet, on the whole, those animals which breathed the mixed vapour were less affected than the others. The smell of the mixed vapour is somewhat peculiar, whether merely resulting from the mixture of the two, or from some new compound, I am unable to say, probably only the former; if from the latter, it also is poisonous, for it must be observed the effects were the same in kind, being only less violent in degree, and therefore, as in practice, the poison and the remedy could by no possibility be so used, I think all that can be fairly deduced in favour of ammonia is, that as it may be beneficial we ought to have recourse to it, not with the expectation of deriving benefit from it in severe cases, but that in those cases where the dose has been so small as to render it uncertain which way the balance will turn it may assist it to incline on the side of life. Experiment 95, where the vapour of ammonia was administered, when the animal had only been exposed for the space of one minute to that of not a strong dose of the acid, and experiment 96, where the whole system was, as far as can be, saturated with the ammonia, *before* the acid was given, will warn us against the expectation, in ordinary cases of poisoning in man by hydrocyanic acid, of deriving any very marked assistance from its use.

I have not as yet referred to the experiments where more than one antidote or remedy were used, as I have been wishful to ascertain, if possible, the degree of value each one possessed. In practice, however, it may be perfectly proper to combine two or more. No. 59 will show the effects of emetics and abstraction of blood; No 33, of the douche and ammonia; No. 74, of ammonia and the douche; No. 79 of ammonia, bleeding, and cold affusion; No. 56, of ammonia and chlorine; Nos. 20 and 47, of nitrate of silver and ammonia, given under the most favourable circumstances, yet without much effect; and others of other combinations. No. 56 is one of the most remarkable experiments I have witnessed in the singular effects of hydrocyanic acid upon the dog. That the case should not have proceeded to a more legitimate termination mortified me greatly at the time, but from what I have since seen, I have but little doubt that the dog would ultimately have recovered. This, with No. 79, might lead at first sight to the supposition that in both the remedies used had been successful. I am sorry that I cannot think so. It would be satisfactory to consider the means used as having been the cause of the continuance of life. This, however, I cannot do; the phenomena arose solely from the inefficiency of the dose. In both, the dogs were large, and the quantity of acid comparatively small, while the symptoms soon after the acid was swallowed evidently indicated that the system was not fully under the influence of the poison. To have discovered an antidote, or found out a remedy, or at least to have confirmed previous suppositions, would have been far more satisfactory than to arrive at the conclusion I fear I must do,—that when the dose of the acid has been large, its effects are such as to completely set at defiance all our means of preserving life, even though we were provided with them for immediate application; and that even where the dose has been much smaller, we have no antidotes, while our remedies are of little efficacy, as compared with those which we can avail ourselves of when other agents destructive to life have been used. However, the surest safeguard for the future accomplishment of this desideratum, if it be within our means, is to know our exact position; to know, in fact,—to use a paradoxical expression,—what we do not know.

I had, while making the observations upon the effects of hydrocyanic acid when administered to living beings, originally intended, among other modes of giving it, to have injected it into the blood; but on seeing that the blood of animals who had been poisoned by it, as well as blood to which the acid had been added external to the body, did not, so far as could be judged of either by the naked eye or by the microscope, in its physical or vital properties differ in any essential particular from healthy blood, this mode of introducing it appeared to be of less importance However, on reflection, I could not but regret that I had not done so, as by it might be elucidated more than one interesting

fact, as well as, possibly, the confirmation or disproof of the opinion I had arrived at from previous experiments, as to the annihilation of the circulation, and the dependence of this upon a persistent contraction of the left ventricle of the heart. The being unable at the time to procure a supply of dogs, which, of all animals, are on many accounts the best adapted for this kind of experiment, and the printing of this volume of *Transactions* at an earlier period than hitherto has been usual, must be accepted as my excuse for the non-incorporation of the following experiments and observations in the body of the paper.

What I more particularly wished to ascertain was, if there were any essential difference in the effects produced by the acid when injected into the blood from those produced by it when introduced into the system by any other means. The comparative intensity and rapidity of its action when so introduced, and its effects upon the blood if it pass into the general circulation; if it enter, in how short a time its presence can be detected in a distant blood-vessel; and whether, if it be found in the blood, the quantity of it progressively, within a certain time, increases. Lastly, it seemed desirable to ascertain if the acid is readily carried off by any of the secretions, as the urine, a mode by which some of the mineral poisons, and many other materials, appear to be carried out of the system.

These experiments sufficiently prove, I think, that hydrocyanic acid does not produce any other effect when injected into the blood than when administered in any other way, the only difference being a somewhat speedier effect, and also, I think, somewhat more decided, so that probably a rather smaller quantity would destroy life by injection than when applied upon a membrane, though, when all circumstances are taken into consideration, I doubt whether absolutely the same quantity coming into *actual* contact, over an equally large surface, upon a mucous membrane, whether alimentary, respiratory, or even the conjunctiva, would not produce, if not as speedy, at least as decisive effects as when injected into the blood. They also confirm what has been stated at pages 138 and 139, as to the acid not acting upon the structure of the blood, for here not any difference could be seen in the characters of the blood; its colour and coagulation was as usual.

That the acid is rapidly carried throughout the vascular system is decidedly shown by the last experiment; for though we might, to avoid the possibility of dispute as to the blood having actually passed the circuit of the lesser and greater circulations, not rely upon experiment 151, where the blood was taken from the same vein, though I have no doubt of the correctness of it, experiment 152 is conclusive upon the question. Here we have positive proof of the presence of the acid in the opposite jugular in one minute after its injection, if not in fifty-five seconds, as those who witnessed these experiments fully believe; and short as the time appears, it is long when compared with what Hering states he found in his experiments to be required for detecting the presence of ferro-cyanate of potass in the jugular vein of a horse after it had been injected into that on the opposite side, which he states to be from twenty to thirty seconds. Now the heart of the horse does not in health pulsate more rapidly than from thirty-six to forty times per minute, while that of the dog beats about ninety-five times; so that, supposing other things be equal, the blood in the latter ought to circulate with at least double the rapidity which it does in the former.

I was anxious to ascertain if the successive portions of blood contained a progressively increasing quantity of the acid, and also if the blood flowed more rapidly, or even continued to flow as rapidly as on the first opening of the vein; because if this were so, it would tend to disprove the correctness of the opinion I have given at pages 141, 145, and 146, as to the condition of the heart, especially of the left ventricle, and the suspension of the circulation. However, these experiments fully confirm the deductions which I had drawn from the previous experiments and *post-mortem* examinations. Experiments 151 and 152 show that, even with such large veins as the internal jugular in No. 151, and the external in No. 152, opened in convulsed and struggling dogs, in the space of a minute, and a minute and a-half, the blood almost ceased to flow. So also in the cat, No. 143, where I am doubtful as to the acid being injected into the vein; in him, the blood was escaping very freely from the femoral vein until symptoms of the acid taking effect were shown, when all hæmorrhage immediately ceased, which, I apprehend, could only have occurred from the pressure of the approaching column of blood (*vis a tergo*) being destroyed, from the left ventricle ceasing to propel any into the aorta.

The evidence is against the presence of hydrocyanic acid in the urine, for though there appeared to be faint traces of the acid in the urine in No. 147, yet no traces were detected afterwards in the urine of the same dog, No. 150, voided three minutes after a full dose of the acid had been given by the mouth, nor in No. 152, where the urine was taken from the bladder thirty minutes after the acid had been injected into the blood, and the presence of which was proved in the blood which had circulated through the body. I should rather rely upon these two experiments, which were carefully made, and conclude that what was supposed to be a faint trace in No. 147 arose from some other cause than hydrocyanic acid, possibly ammonia.

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *Observations on the Use of the Microscope in Investigations connected with Anatomy, Physiology, and Pathology; being the substance of the Annual Oration, delivered before the Medical Society of London, on the 8th March* 1844. By THOMAS BELL, F.R.S., &c., Fellow of the Royal College of Surgeons of England; Professor of Zoology in King's College; and President of the Microscopical Society. (Transactions of the Medical Society, London. New Series, vol. I., p. 1–14; London, 1846, 8vo.)

2. *A Practical Treatise on the Use of the Microscope; including the different Methods of Preparing and Examining Animal, Vegetable, and Mineral Structures.* By JOHN QUEKETT, Assistant Conservator of the Museum, and Demonstrator of Minute Anatomy at the Royal College of Surgeons of England. Illustrated with Nine Plates and Two Hundred and Forty-one Wood Engravings. London, 1848. 8vo, pp. 464. (Library of Illustrated Standard Scientific Works. Vol. VI.)

A CONSIDERABLE degree of truth must be conceded to the observation of Mr Bell, that it is possible to trace, in the history of the study of physiological science, certain epochs, at which fashion had no small influence in determining the aspects under which its phenomena have been investigated, and the means which have been principally employed in conducting the requisite researches. At one period the dissection of animals, the examination of comparative structures, and the inquiry into the relation subsisting between animals of a low and those of high forms of organization, occupied, he thinks, not exclusively, but certainly in a disproportionate degree, the attention and study of physiologists. This admission, proceeding from one so well known as an excellent comparative anatomist, deserves serious consideration. At another period, those branches of physiological knowledge which might be elucidated by experiments upon living animals, either in the healthy state, or under morbid and unnatural conditions artificially induced, occupied, he thinks, an undue proportion of attention; while they are always exposed to the objection of being more or less imperfect, of giving limited and perhaps erroneous results; and their performance implies a degree of cruelty which cannot be easily contemplated. By a third race of physiologists, the laws of life and organization have been studied through the medium of chemical phenomena and by the aid of chemical analysis. Of these inquirers, Mr Bell says, probably with some harshness, that they sought, in the experiments of their own comparatively coarse and clumsy manipulation, the analogies and explanations of the delicate processes incessantly going on within the laboratory of nature. Each of these methods of investigating physiological phenomena, Mr Bell justly observes, has its own advantages; and all of them are highly serviceable and important. But they require to be employed, not separately, and to the exclusion of each other, but in close relation to each other, and so as to render each auxiliary to all, in promoting the common objects of inquiry.

During the last ten or twelve years these several means of obtaining information have been gradually and comparatively sinking in importance before the employment of the microscope; and though comparative anatomy, experimental physiology, and chemical analysis have still cultivators, yet the number of these is small, compared with the multitudes who, in prosecuting their respective inquiries, are in the daily habit of using the microscope.

The circumstances which have led to this revolution are perfectly well known. The microscope was at all times, in the history of anatomy and physiology, an instrument in more or less general use, at least among eminent anatomists. Malpighi at an early period had recourse to it in those investigations into the minute structure of animals and plants, which are still quoted with respect. Grew, in like manner, employed the instrument much. It was used by William Cowper, by James Keill, by Leewenhoek, Swammerdam, Lieberkuhn, Henry Baker, George Adams, William Hewson, William Cruikshank, the second Monro, Fontana, Spallanzani, Mascagni, and others too numerous to specify. The instrument, however, as employed by these observers, was of rude and imperfect construction, in no instance perfectly achromatic; and among those who employed it were occasionally some, who were not sufficiently experienced to distinguish, amidst optical illusions, the true appearances, and to give to the appearances presented the true interpretation. With very imperfect instruments, nevertheless, several of these observers were able to discover facts, the correctness of which has been amply confirmed by subsequent inquirers,—a circumstance which proves that it is not in all instances the instrument, so much as the man who uses it, and who knows how to use it, that renders the microscope a serviceable and faithful guide. The history of science records the labours and discoveries of Leewenhoek, Swammerdam, and Lieberkuhn in the recondite recesses of animal anatomy; of Thomas Bradley, of Henry Baker, and of George Adams in the secrets of insect life; of William Hewson in the blood globules and the absorbent system; of William Cruikshank in the history of the lacteals; and of Paul Mascagni in the absorbent system and histology in general. The researches of these men were made by the aid of instruments, which, though the best that the artists of the time could produce, were infinitely inferior to those of the present day. Yet are the observations of these inquirers, so far as they go, wonderfully correct; and if we compare their means of research with those which we enjoy at the present day, it will appear more wonderful, that they penetrated into so many of the secrets of nature, than that they learned any thing accurate regarding objects too minute for the vision of the unaided eye.

It may be well to advert shortly to the nature of these instruments.

The single microscope, or the common magnifying glass, consisting of one or two lenses, through which the object is directly beheld in a magnified form, was the first sort of instrument, and that employed by the earliest observers. This, in the form of a small double convex lens, was the microscope employed by Leewenhoek in the early part of his researches. A very powerful modification of these were the glass globules by which it has been stated that Leewenhoek made most of his great microscopical discoveries. This, however, according to Baker, is not the fact. The microscopes employed by Leewenhoek, indeed, consisted each of one single lens set between two plates of silver, perforated with a small hole, with a moveable pin before it, on which the object was to be placed and adjusted to the eye of the beholder. Of twenty-six microscopes left by Leewenhoek at his death to the Royal Society, each instrument has a double convex lens, and not a sphere or globule. All the parts of these microscopes are of silver, and fashioned by the hand of Leewenhoek himself; and the glasses, which are represented to be excellent, were all ground and set by himself, each instrument being devoted to one or two objects only, and applicable to nothing else. In this manner he had constructed a microscope, with a glass adapted to almost every object, until their number amounted to several hundreds. The highest magnifying power was one hundred and sixty diameters; the lowest, forty.

The glass globules were not free from objections. The great extent to which the rays of light in passing through these globules are refracted, necessarily magnified much any object which was viewed through them. But they magnified irregularly, and consequently required frequent changing of the field of vision, in order to view every part of the object under the same powers.

These glass globules, it is believed, began to be employed as lenses for the single microscope about the year 1665, and there is good reason to think that their employment was first suggested by Dr Robert Hooke, who describes the manner of making them in the preface to his MICROGRAPHIA ILLUSTRATA, which was published in 1656. These globules Hartsoeker appears to have employed in examining and describing the spermatic animalculæ about the year 1674; and subsequent to this period, they were more or less used, probably by Swammerdam and Lyonnet, and, indeed, by all the dexterous microscopical observers during the first half of the eighteenth century. Of these globules, Father della Torre of Naples made great and most successful use in studying the minute structure of the brain, and many other objects requiring high magnifying powers. Della Torre was also most dexterous in the manufacture of these globules, and appears to have understood this department of art better than any of his contemporaries. From his account, and other sources, the mode of preparing them, the methods of mounting and using them, are fully described, by George Adams the younger, in his Microscopical Essays.

In the year 1740, a great improvement on the single microscope was effected by Lieberkuhn, who contrived a highly polished concave speculum of silver, and, by placing in the centre or focus of this a small lens, he was enabled to direct, by reflection, a strong light upon the upper surface of the object, and thus examine it in a deliberate and satisfactory manner. This form of mirror is still called, from the author of the contrivance, the Lieberkuhn.

The single microscope, from its portability and easy mode of application, was always in great use with anatomists, naturalists, and botanists. One form, for the use of botanists, has been long known, and is convenient for studying the structure and parts of many minute objects.

The effects of the single microscope unquestionably suggested the construction of the compound microscope; in which not only were the parts of the object magnified by object glasses, but there was an intermediate lens to concentrate the radiating and refracted pencils of light, and an eye-glass more or less deep. One of the first, if not the first, compound microscopes constructed in this manner is described by Hooke in his MICROGRAPHIA in 1656. Of another similar instrument, constructed by Eustachio Divini, an artist at Rome, an account was communicated to the Royal Society in 1668.* This appears to have been a large instrument;

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* Philosophical Transactions, No. 42. London, 1668.

probably too large to be easily managed. It had four separate lengths. When closed, it was sixteen inches long, and magnified the diameter of an object forty times; at the second length it magnified ninety times; at the third length, one hundred and eleven times; and at the fourth length, one hundred and forty-three times. The eye-glasses, which were two plano-convex lenses, were so placed as to touch each other in the centre of their convex surfaces; by which means the glass embraces a greater extent of the object, the field is enlarged, the extremities less curved, and the magnifying power is greater. The tube in which the glasses were enclosed was as large as the leg of a man, and the eye-glasses were as broad as the palm of the hand. The object lenses were not varied.

At the same time, S. Campani, an artist of Bologna, contrived a microscope somewhat similar to that of Divini, being on the principle of an inverted telescope. The lenses made by Campani are said to have been worked as a turning tool, and not moulded. If so, he must have acquired considerable dexterity in working with glass. In 1672, microscopes in imitation of these two artists were manufactured by G. P. Salvetti, and which were found to be far superior in their magnifying and defining powers. On the peculiarities of construction in these instruments, and by what means they were thus made to excel those of Divini and Campani, however, no information is given.

In 1698, Philip Bonnani published an account of two microscopes, both compound, and both consisting of three sets of glasses,—the object lens, the middle glass, and the eye-glass. The second microscope was peculiar in being provided with a small tube, in each end of which was inserted a convex lens, the use of which was to concentrate, by the aid of a lamp, the rays of light on the object, and thus to obtain a more perfect degree of illumination. A good figure of this second form of microscope is given at page 8 by Mr Quekett.

After this period no considerable improvements were made in the compound microscope till the year 1736; and it is the opinion of the younger Adams that the microscopes of Hooke, Divini, and Bonnani, were as perfect as any of those which had been constructed down to his own time, and that after the time of the latter artist little real improvement had been effected. Of all these microscopes, none were achromatic; and objects seen were all more or less chromatized.

Sir Isaac Newton had, previous to 1672, investigated the laws of light and colours, and made known in that year the new views with which his optics abound, and the applications which they suggested in the construction of optical instruments. The reflecting telescope led him to apply the same principles to the microscope, and he seems, in 1672, to have contrived the first compound reflecting microscope. Many years, nevertheless, appear to have elapsed before opticians reduced this principle to practice, and more than one-third of the eighteenth century appears to have elapsed before attempts were made sufficiently important to demand being recorded.

About 1736, Dr Robert Barker contrived a microscope upon the principle of the reflecting telescope, provided with two specula; and Dr Smith, the author of the Treatise on Optics, one with a concave reflecting speculum and a convex reflecting speculum, which is stated to have performed very well.

Three forms of microscope were thus in use among naturalists and entomologists,—the single, the compound, and the compound reflecting; and as all acted equally well in proper hands, attention was divided nearly equally between them. The compound microscope was objectionable, nevertheless, by its large size, and its consequent small degree of portableness; while, excepting by Stephen Gray, and Dr Barker and Dr Smith, the reflecting microscope was not much used. The single microscope, on the other hand, being more portable and more easily managed, was a more general favourite; and a pocket form of this, proposed in 1702 by Mr J. Wilson, continued for many years to be the instrument most generally employed.

It is not easy to say at this distance of time,—more than one century and a quarter,—whether, if the attention of opticians had been adequately directed, they might have greatly improved either the compound or the compound reflecting microscope. Meanwhile, a particular circumstance diverted for a time the thoughts of microscope manufacturers in a new direction.

In the year 1738, Lieberkuhn made known his invention of the solar microscope, in which not the object itself but its image is seen, while the sun is shining, and its rays are received on a concentrating lens. The use of this microscope, therefore, was confined not only to a particular time of the day, but to clear, unclouded weather, and only to the examination of transparent or nearly transparent objects, or those which could be rendered so. The magnifying and representing faculties of this instrument were nevertheless so remarkable, that for a long time by it all attention was engrossed; and while it was generally employed by observers, especially entomologists and draughtsmen, artists devoted much time and skill in rendering it as perfect as possible. It was greatly improved by Mr Cuff, an artist of the day, who applied to it a moveable mirror or speculum, and thus rendered it more capable of general employment.

The solar microscope, as improved by Mr Cuff and others, continued long in use, and, indeed, has never been entirely laid aside by those who studied, and wished to exhibit to others, the structure of animalcules, insects, and the circulation of the blood in the thin and semi-transparent parts of cold-blooded animals. By its means, almost all the drawings and representations of magnified insects and animalcules were made during the course of the eighteenth century. The solar microscope, however, is incapable of being used for opake objects, and hence the simple or the compound microscope was the one still employed for these purposes.

By far the most able microscopical observer of this period was John Nathaniel Lieberkuhn, already mentioned. Lieberkuhn was a Prussian, born at Berlin in 1711, who had graduated at Leyden, and was afterwards settled at Berlin as professor of anatomy. The instrument most commonly employed by Lieberkuhn was a variety of the single microscope, somewhat similar to the pocket microscope of Wilson, consisting of an object lens, an intermediate speculum perforated in the centre, an object holder, and a concentrating lens. Of this sort of microscope, twelve specimens, with preparations fixed within them, are still preserved in the Museum of the College of Surgeons of England.

Lieberkuhn was eminent as an anatomist and injector of the minute parts of the alimentary canal, and is the first who gives a correct description of the true shape and structure of the intestinal *villi.* The memoir containing the account of his researches was first published in the memoirs of the Royal Academy of Berlin in 1744; and in the following year he published in the same work the description of his anatomical microscope, with two illustrative figures, and ample directions for examining not only the most minute parts of animal bodies after death, but for fixing and examining even the parts of living animals. This expert anatomist died on the 7th December 1746, at the early age of forty-five. During his short career he had made, with his own hands, a collection of more than four hundred preparations of minute anatomy, almost all microscopical, and which continued on sale at Berlin and other places for a long time. Portal states that he could not learn into whose hands the collection had come. We have understood that it was sold in detached portions, and dispersed in several different quarters. A number were purchased after the death of the son of Lieberkuhn by Professor Beireis of Helmstaedt, and are still preserved there. The twelve preparations above mentioned originally belonged to the collection.

Three years after his death,—namely, in 1749,—another memoir of Lieberkuhn, On the Proper Methods of Discovering the Structure of the *Viscera*, was published in the fifth volume of the Memoirs of the Berlin Academy. These dissertations, with another, were subsequently republished in a collected form in 1782, by John Sheldon, the London Teacher of Anatomy.*

The inconveniences of the compound microscope as a cumbrous and unwieldy instrument were so strongly felt, that various attempts were made by such makers as Mr Cuff, Mr Culpeper, and Mr Scarlet, to render it more portable and more manageable. Even after these artists had exerted much ingenuity in improving the instrument, we find Henry Baker, a great microscopical observer and entomologist, complaining in 1744 of the difficulty of adjusting its parts, of the legs or supports of the instrument coming in the way of observation, and of its being destitute of any good contrivance for viewing opake objects. These inconveniences were in a great degree obviated by Mr Cuff, and more completely, apparently, by Mr Benjamin Martin.

About this time, 1740–70, the metropolis possessed a philosophical instrument maker of much skill and considerable mechanical ingenuity,—namely, George Adams,—who may be named the elder, to distinguish him from a son not less eminent. This artist, who had bestowed much study in the manufacture of microscopes, published in 1746 a treatise entitled, like that of Hooke, MICROGRAPHIA ILLUSTRATA, or the KNOWLEDGE OF THE MICROSCOPE EXPLAINED; in which he gave a description of the construction, magnifying powers, and uses of microscopes in general, and full directions for the best methods of preparing, mounting, examining, and even preserving all such minute objects as require microscopical examination, in order to elucidate adequately their figure and structure. This was the first systematic treatise on the microscope published in this country; and collecting, as it did, from all sources, a reasonable amount of information, contributed greatly to make known the faculties and capabilities of the instrument, and to promote the cultivation of that species of knowledge which is obtained by the microscope. George Adams appears to have increased greatly the powers of the compound microscope, chiefly by increasing the number of lenses, which were six, and arranging them in such a manner on a large plate of brass, that the latter being capable of being turned upon the central pillar of the instrument, each lens could in succession be brought beneath a hollowed plate or cup which served as an eye-piece. For ordinary adjustment, this plate was made to slide up and down the pillar, thereby altering the *foci;* and, for fine and accurate observations, a screw was employed, by turning which, that portion of the pillar to which the stage was attached, was slowly raised and depressed. Besides these microscopes, which may be regarded as the invention of George Adams himself, he was in the habit of making microscopes after the fashion of Wilson, Lieberkuhn, and Culpeper, all of which are described in the work now mentioned.

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* Dissertationes Quatuor Anatomicae, Auctore J. N. Lieberkuhn, curâ J. Sheldon. Londini, 1782. 4to.

This was the era of M. Trembley of Geneva, Mr Henry Baker, Mr John Ellis, and, it may be added, the theories of Buffon and Needham. Buffon was himself little of an observer; but he possessed the valuable talent of converting to excellent account the observations made by others. His active imagination, and his glowing eloquence, however, have so confounded the objects which he undertakes to describe, that various microscopical observers have arrived at the conclusion, that he never beheld any of them, and must have borrowed the accounts at second hand.

At this time, also, flourished John Hill, a botanist, and writer on the anatomical structure of trees, of considerable eminence. His work, exhibiting the structure of the Wood by the Microscope, which appeared at London in 1770, attracted much attention, and directly tended to recommend the use of the microscope as a means of research into the minute structure of organized bodies. Mr Hill employed the compound microscope, and gives a view of the instrument which he used and the method of using it. His work contains many instructive facts, and will at all times be studied with interest by the student of natural history.

The MICROGRAPHIA of the elder Adams was a popular work, considering its subject. In the course of twenty-four years, three editions were exhausted, and a fourth was published by the author in 1771, in which he introduced accounts of the principal contrivances that had been made during the previous twenty years, and the description of a contrivance by himself for accommodating the solar microscope to the *camera obscura*, and illuminating it at night by means of a lamp. This modification was afterwards still further improved under the name of Lucernal Microscope by his son, and was made capable of exhibiting opake as well as transparent objects.

It ought not to be omitted, that the solar microscope was greatly improved by Benjamin Martin, and was made capable of showing on a screen a magnified image of the surface of opake bodies.

The present was the era of William Hewson and his pupil and relative Magnus Falconar. It was in May 1770, that Hewson communicated to the Royal Society his two Memoirs, on the Blood and its Morbid Appearances, and on the Coagulation of the Lymph and the Serum, and, in June 1773, the Essay on the Figure and Composition of the Red Particles of the Blood. The instrument which Hewson chiefly employed was the single microscope; and Falconar speaks of it as if he had employed it exclusively and preferably.* Yet by this instrument he was enabled to anticipate the knowledge of some of those facts, the full establishment of which is regarded as the greatest triumph of modern microscopical science. Hewson was well acquainted with the *corpora globosa* or Malpighian vesicles of the kidney, and their peculiar structure,† as consisting of multiplied minute convoluted arteries. He was aware of the celluliform structure of the mucous membrane of the stomach, which has been subsequently so fully explained by Dr Boyd.‡ He had examined most carefully the intestinal *villi* in men and animals, and explained the connection between them and the lacteal vessels. Some of these preparations he had adapted to microscopes after the manner of Lieberkuhn.§ And, lastly, he was the first who described accurately the figure of the blood-corpuscle, showed that it is not a globule or spherical, but a flattened spheroidal vesicle, and represented it to be what is now called a nucleated cell.||

It may, indeed, be said, without exaggeration, that William Hewson was either the most skilful or the most fortunate microscopical anatomist, or both, that England has produced; and it is scarcely too violent an inference to think, that had his life been prolonged to the usual period, he might have entirely changed and renovated the science of minute anatomy. It is interesting to observe that Mr Thomas Bell admits the remarkable merits of this ingenious and accurate anatomist.

Magnus Falconar appears to have employed exclusively the single microscope, as described by Henry Baker. He states that he used occasionally the small glass spheres of Father della Torre. But as it was impossible to get them made in London pure and uncontaminated by the smoke of the light employed, they were little used. Falconar found the lenses manufactured by Mr

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* Experimental Inquiries, Part the Third; containing a Description of the Red Particles of the Blood in the Human Subject and in other Animals, &c.; being the Remaining Part of the Observations and Experiments of the late William Hewson, F. R. S. &c. By Magnus Falconar, Surgeon. London, 1787. 8vo. Pp. 4 and 5.

† "But I have repeatedly observed, and have now by me some preparations which prove that these *corpora globosa* are not uniform bags, but convoluted arteries, which comes nearest to the idea that Ruysch had of them. Some ingenious anatomists have warmly espoused a contrary opinion, and have not only supposed the kidney to have follicles, but most other glands of the body, particularly the breast or *mamma*, and the salivary glands. But that they likewise have been deceived by a cluster of small vessels, will appear probable, when we consider that the *corpora globosa* in the kidney, which have by so many been pronounced to be bags or follicles, are only small vessels clustered together or convoluted."—Experimental Inquiries, Part II. Chap. xii. p. 178. London, 1774.

It deserves notice, that, in some respects, both parties are right. The *corpora globosa* have been shown to be minutely divided arteries, convoluted and arranged without and within a capsule or vesicular bag.

‡ Ibid. Chapter xii. p. 173.

§ Ibid. p. 182.

|| Experimental Inquiries, Part III.

Baker to be the most suitable for examination of the blood-corpuscles. Like all good observers, he remarks that the London lenses, though inferior in power, more than compensated for this by their greater distinctness.

Next to Hewson may be placed William Cruikshank and John Sheldon as microscopical anatomists. Cruikshank gave correct and instructive views of the intestinal villi and their structure, of their connection with the lacteals, of the lacteal vessels themselves, and the most obscure parts of the absorbent system. He also appears to have been the first who employed the microscope to discover the structure of the skin, and the relations between the true skin and the scarf skin.

To the same period may be referred Fontana in Italy, and Prochaska at Vienna. Both of these authors, and especially the latter, studied carefully by the microscope the structure of many parts of the nervous system, and anticipated several of the facts which were more fully brought to light in the present century by the researches of Bauer, Ehrenberg, Remak, and Valentin.

Of Mascagni and his microscopical labours, we scarcely know how to speak. It is impossible to doubt that he was a most diligent anatomist, and that he studied most carefully both the peculiarities of the lymphatic system and histology generally. His PRODROMO, or introduction, which is devoted to this subject, is a work of great labour and research, and not merely one of literary research, but actual personal anatomical inquiry. Yet, in those parts founded on microscopical observation, it contains many statements which are not only at variance with the results of subsequent inquiry and representations, but which show that he was not on his guard against the illusions to which at that period, certainly, microscopical observation was particularly exposed. Mascagni has not only the serpentine cylinders of Monro and Fontana, but several other singular enough pieces of creation. Some of these mistakes may doubtless be ascribed to the imperfect instruments then in use. But, unquestionably, not a few were owing also to the incapacity of the observer, at least his incapacity to distinguish between appearances presented by the microscope and the interpretations to be put upon these appearances. The plates to the PRODROMO, as well as the text of that elaborate work, afford ample confirmation of the truth of these remarks.

By this time George Adams was advanced in life; but his son began to follow in the footsteps of the father. George Adams, the younger, published in 1787 his Essays on the Microscope, in which he not only embodied the information contained in the MICROGRAPHIA of his father, but introduced a large amount of new matter on recent improvements in the Microscope, on the Theory of the Instrument, on the best Method of making Observations, on the Modes of mounting objects for Examination; a condensed View of the Structure and Transformation of Insects as exhibited by the Microscope; an Account of the Anatomical Structure of the *Cossus* or Willow Caterpillar; the Natural History of the *Hydra*, or Fresh Water Polype; the History of Infusory Animalcules; the Organization of Timber; and the Crystallization of Salts as seen by the Microscope.*

This is a most instructive treatise, and continued long to be the great authority on microscopes, microscopical observation, and microscopical objects. It contains, indeed, in a very clear and unassuming manner, a large amount of useful and entertaining information; and cannot be perused even in the present day without great advantage. The author was a person of extensive and accurate scientific attainments, well acquainted with the labours of his predecessors and contemporaries, deeply impressed with the benefits of correct and minute knowledge, both in physics and in natural history, and, with much benevolence, eager to discover and explain to others the adaptation of means to ends. Of his success in this undertaking, which is not always easy, and not free from hazard, different opinions may be entertained by different persons. But of the excellence of his objects and their general good tendency, it is impossible for the most skeptical to entertain any doubt.

The part of the work devoted to the description of the different forms of the microscope is very ably done. It contains ample and correct information on these instruments up to the time at which the work was published. Among others, the author describes a form of the lucernal microscope of his father, which he had so much improved as to render almost a new instrument.

Of the merit of this work it is probably the least equivocal proof, that its appearance may be taken to indicate a fixed point in the history of the uses and applications of the microscope. The example of Hewson was enough to induce other anatomists to follow in the track marked out at that time. Yet, excepting in the case of the second Monro, who was his contemporary, the continental observers already mentioned, and Soemmering, who belongs to a period a little later, not many were induced to study anatomical structure in this manner. The microscope was either improved to so great a degree that no more improvement could be effected on it; and it was stationary, yet from various circumstances was not susceptible of general employment; or it was an instrument on which reliance could not be placed, unless in the hands of most practised and skilful observers; persons who were as well skilled in optics as in anatomy, and capable of picking, as it were, their path amidst snares and sources of error. These considerations evidently deterred from using the microscope, at least from the systematic and habitual use of the compound microscope, many who, had they studied the work of George Adams the younger, must have seen, that the difficulties were more in appearance than in reality. Let it not be concealed, nevertheless, that hitherto one problem had not yet been solved. There was no such instrument as a perfect achromatic microscope; and all those yet in use chromatized more or less forcibly. It is probable that this and some other optical objections did operate in preventing the use of the instrument from becoming general among practical anatomists.

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* Essays on the Microscope, containing a Practical Description of the most Improved Microscopes: A General History of Insects, their Transformations, Habits, and Economy; an Account of the Species and Properties of the *Hydrae* and *Vorticellae;* a Description of three hundred and seventy-nine Animalcula, &c. &c. By George Adams, Mathematical Instrument Maker to His Majesty, &c. London, 1787. 4to.

During all this period,—for the last ten years of the eighteenth century and the first twenty years of the nineteenth,—other modes of exploring the intricacies and the difficulties of the animal economy were, as Mr Bell has stated, more in credit with the physiological world. Chemistry was a popular study, promised much, and in good hands had done great things. The use of the microscope, in short, was confined principally to the great ones in anatomy and physiology; and the task of observing, by its means, was either believed to be so difficult, or was beset by so many fallacies, that, on the one hand it was not general, and on the other, all descriptions professed to be taken, especially from examination by the compound microscope, were received with doubt and some degree of suspicion; and it was rarely found that the descriptions of two observers agreed.

A considerable degree of this discredit was, it must be allowed, brought on microscopic observations by mistakes arising from imperfect acquaintance with the science of optics, and probably instruments provided with inferior glasses. An eminent lecturer on anatomy in this city was in the habit of amusing his audience by giving rather ludicrous descriptions of the mistakes committed by the second Monro, who imagined that he had resolved the ultimate fibres of animal bodies, vegetable productions, and minerals into aggregations of serpentine fibres. This gentleman impressed his pupils with the idea, that microscopical observers could see, in the objects of their examination, anything for which they took a fancy, and that all microscopical anatomy was very much a matter of imagination. It is curious that it did not occur to this gentleman, who was in other respects an excellent man, a person of scientific attainments, and a most able teacher, that there might be an abuse of a thing as well as a use, and that abuses, misapplications, and errors, do not constitute well-founded arguments against the uses and the judicious applications of any instrument or any art.

During all the period, in short, between the appearance of the work of George Adams, junior, and the first ten or twelve years of the nineteenth century, the compound microscope had undergone, in its optical arrangements and faculties, no essential change,—no improvement,—and any modifications which it had received related chiefly to its mechanical appliances,—the methods of moving and fixing the optical parts, and the methods of viewing and examining the objects,

Between the generation to which we now refer, and that which belongs to the present day, stands Francis Bauer, a name too often veiled and obscured in that of Sir Everard Home. Francis Bauer was a true and genuine specimen of the accomplished microscopical observer of the time. A native of that country where the use of the microscope has never been wholly neglected, and in which it has of late years been universal among anatomists and naturalists, where patience and unwearied diligence are the peculiar character of philosophers, Francis Bauer turned all these national and personal qualities to the duty of microscopical observation and the study of histological anatomy. Bauer, however, was little more than an observer, and indebted, as we are, to Sir Everard Home, for making known almost all the results of his multiplied researches; we know little or nothing of his modes of examination or his peculiar discoveries. It is not easy to say whether the errors which they contain should be ascribed to Sir E. Home or Mr Bauer.

It is probable, it may be said certain, that the instruments employed by Francis Bauer were not better than those in common use during his time. If we estimate this space from the commencement of the present century to the year 1828, distinguishing the space into four periods of seven years each, then it may be said, that, during the three first periods, the course of artistical and mechanical progress as to the microscope was almost stationary, and that, whatever was done in theory and science, it was not made susceptible of application till the fourth and last period. During the early periods of this space, Sir David Brewster had, by combining experiment with science, discovered several valuable facts, more especially the high magnifying powers of the diamond, sapphire, garnet, ruby, and other gems, when cut and ground into the shape of lenses, and the effects of various combinations of fluid and semi-fluid transparent substances, which he employed as object lenses. Dr Wollaston had described, in 1812, the effects of the periscopic doublet, under the name of periscopic microscope, a contrivance which was afterwards, in 1821, greatly improved by Sir John Herschel; and soon after, the experiments of Dr Goring and Mr Coddington, on the lenses of the compound microscope, tended to render the subject more accurately known. One defect, still, however, adhered to the compound microscope. The instrument was not achromatic.

It was always well known, that to render the compound microscope achromatic was a most desirable object; and so long ago as 1776, Euler proposed to employ achromatic object glasses in the compound microscope; but to so late a period as 1821, M. Biot considered their introduction as inadmissible, in consequence of the difficulty of preparing achromatic lenses as small as those which are required for the microscope. In 1823, however, M. Selligues of Paris, and Dr Goring of London, both directed their attention to the subject, the former having employed the two MM. Chevalier, since that time well known as opticians and microscope manufacturers; and the latter, Mr Tulley, to prepare small achromatic object-glasses. To M. Selligues, however, belongs the merit of forming the idea to make the object-glass consist of four achromatic compound lenses, each of which was composed of two single lenses. The principle of this construction is the true source of the superiority of the achromatic microscopes, since it may be shown by optical conditions that this combination is, if not the only, at least the most effectual method hitherto known of obviating those refractive phenomena which give rise to chromatism. Professor Amici of Modena, who had attempted, between 1812 and 1815, to construct an achromatic object-glass of one single lens, found that it was impracticable, and was obliged, in order to attain his object, to adopt the plan devised by M. Selligues, of conjoining several object-glasses. In the instrument of M. Selligues, the focal length of each object-glass was eighteen lines, its diameter six lines, and its thickness at the centre six lines. In the instrument of Amici the focal length of each object-glass was about six lines. The MM. Chevalier have since that time constructed them with only two lines of focal length. And, subsequent, Mr Pritchard of London, to whom the modern microscope owes so much, surpassed these artists by making object-glasses of one-sixteenth of one inch in focal length.

On the 5th of April 1824 M. Selligues presented to the Academy of Sciences a microscope constructed on the principle now mentioned of combined lenses by M. Chevalier; and in the course of the same year, and without knowledge of what had been done in France, the late Mr Tulley, at the suggestion of Dr Goring, constructed for a compound microscope an achromatic object-glass of nine-tenths of one inch focal length, composed of three lenses, and transmitting a pencil of eighteen degrees. This was the first achromatic microscope that had been made in England; and its excellence may be estimated by what Mr Quekett observes, that, as regards accurate correction throughout the field of vision, that glass has not been excelled by any subsequent combination of three single lenses. Mr Tulley afterwards made, in order to be placed in front of the lens first mentioned, a combination which increased the angle of the transmitted pencil to thirty-eight degrees, and furnished a power of three hundred diameters. It must not be forgotten, that in thus carrying to perfection the achromatic lenses, Mr Jackson Lister has rendered services which it is not easy justly to appreciate.

"On the 30th of March, 1825, M. Chevalier presented to the Society of Encouragement, an achromatic lens of four lines' focus, two lines in diameter, and one line in thickness in the centre: this lens was greatly superior to the one before noticed, which had been made by him for M. Sellignes.

"In 1826, Professor Amici, who, from the year 1815 to 1824, had abandoned his experiments on the achromatic object-glass, was induced, after the report of Fresnel to the Academy of Sciences, to resume them, and in 1827 he brought to this country and to Paris a horizontal microscope, in which the object-glass was composed of three lenses superposed, each having a focus of six lines and a large aperture. This microscope had also extra eye-pieces, by which the magnifying power could be increased. A microscope constructed on Amici's plan, by Chevalier, during the stay of that physician in Paris, was exhibited at the Louvre, and a silver medal was awarded to its maker.

"'Whilst these practical investigations were in progress,' says Mr Ross, 'the subject of achromatism engaged the attention of some of the most profound mathematicians in England.' Sir John Herschel, Professors Airy and Barlow, Mr Coddington, and others, contributed largely to the theoretical examination of the subject, and, though the results of their labours were not immediately applicable to the microscope, they essentially promoted its improvement.

"For several years prior to 1829, the subject had occupied the mind of a gentleman who, not entirely practical like the first, nor purely mathematical like the last mentioned class of observers, was led to the discovery of certain properties in a chromatic combination, which had been before unobserved. These were afterwards experimentally verified; and in the year 1829, a paper on the subject, by the discoverer, Joseph Jackson Lister, Esq., was read to and published by the Royal Society. The principles and results thus obtained enabled Mr Lister to form a combination of lenses, which transmitted a pencil of fifty degrees with a large field correct in every part. This paper, which was the ground-work of all the great improvements which have been effected in this country in the achromatic object-glasses, has tended to raise the compound microscope from its primitive and almost useless condition, to that of being the most important instrument ever yet bestowed by art upon the investigator of nature, and has gained for the discoverer a lasting reputation. As the results arrived at by Mr Lister are indispensable to all who would make or understand the instrument, I would refer them to the paper itself, which is contained in the 121st volume of the Philosophical Transactions. One of the greatest improvements which Mr Lister introduced into the manufacture of his object-glasses, was the joining together of the plano-concave flint lens and the convex, by means of some transparent cement, such as Canada balsam. 'This,' he says, 'is desirable to be taken as a basis for the microscopic object-glass; it diminishes very nearly half the loss of light from reflection, which is considerable at the surfaces of a combination. I have thought,' he says, 'the clearness of the field and brightness of the picture evidently increased by doing this; it prevents any dewiness or vegetation from forming on the inner surfaces, and I see no disadvantage to be anticipated from it, if they are of identical curves and pressed closely together, and the cementing medium permanently homogeneous.' From this discovery of Mr Lister's, in 1829, we may fairly date the rise and continued progress towards perfection, of the achromatic compound microscope in England; and all cultivators of natural science, as well as the makers of the instruments themselves, are largely indebted to Mr Lister for publishing to the world the valuable results of those labours, which certainly have formed the groundwork of the plan on which all our first-rate opticians now work, for whose success he has always most zealously exerted himself, even to the examination, from time to time, of their wonderful productions; and it is but common justice here to state, that we have now in this metropolis three most eminent manufacturers of the compound achromatic microscope, viz., Messrs Powell, Ross, and Smith, whose instruments are without equal in this or any other country. On consulting the dates at which these opticians respectively commenced the manufacture of achromatic object-glasses, we find that as early as March, 1831, Mr Andrew Ross had completed, for Mr Wm. Valentine, a dissecting microscope on an entirely new plan, being provided with coarse and fine adjustments, and stage movements, and with a Wollaston condenser. This instrument, first described in the forty-eighth volume of the Transactions of the Society of Arts, will be more fully mentioned in the chapter devoted to the single microscope; although generally employed for dissecting, it was nevertheless made capable of receiving a compound body. The first microscope of this kind made by Mr Ross, is now in the possession of R. H. Solly, Esq., for which, in 1832, Mr Ross was also employed to construct a triple object-glass, he, previous to the year 1831, having made lenses of the precious stones, and acquired his knowledge of achromatism in connection with Professor Barlow, having operated for the professor during the construction of his fluid object-glass, and also in the arrangement of his formula for computing the radii of curvature of an achromatic one."—Pp. 38–41.

On the services of Mr Jackson Lister, Mr Thomas Bell pronounces a well-merited panegyric:—

"To this latter gentleman I have already slightly alluded; but this is the proper place to speak more distinctly of his great merit, and to acknowledge, with suitable gratitude, the obligations we all owe to his talents, his industry, and his liberality. It is, indeed, to him,—to this London merchant, occupied in his honourable pursuits of business during his days, and devoting his leisure moments, in the most disinterested manner, to the pure and healthful relaxation of scientific research,—for to such a mind, study was the most delightful relaxation,—it is, I say, to this gentleman that we owe, in a great measure, the enthusiastic revival of microscopic research, which has distinguished the last fifteen or twenty years. The discovery of the true principle of achromatism was not the work of a professed philosopher,—it was not the result of the studies and abstract calculations of a college recluse, nor of the experiments of a practical optician. It was the amusement of the leisure hours of a busy London merchant, and was perfected amidst all the active pleasures of a large and happy family. But this was not all. By directing the energies of practical opticians, and enabling them to carry out his views, he laid the foundation of the fame and success of several of the most distinguished manufacturers of microscopes of the present day, who have reason gratefully to acknowledge the genius and liberality of this excellent person."—P. 11.

"Amongst those in this country by whose agency the microscope has been much improved, may be mentioned the names of Mr Varley and Mr Pritchard, both of whom are well known to the microscopic world by their valuable publications. To the former, in 1831, we are indebted, first, for a microscope with a lever stage movement, for following animalcules, together with capillary cages for containing the same, fishing tubes and other apparatus equally ingenious and useful, and for his lathe for grinding and polishing lenses; secondly, for his vial microscope, for viewing the circulation in chara; thirdly, for his graphic telescope and microscope; fourthly, for his valuable instructions and hints concerning the best forms of eye-pieces for telescopes and microscopes; and, lastly, for his improved lever microscope,—all of which inventions have been fully described in the Transactions of the Society of Arts, to which interesting papers I would refer my readers. To the latter, Mr Pritchard, we are indebted for three valuable works on the microscope, viz:—The Microscopic Cabinet, The Microscopic Illustrations, and The Micrographia,—in which are admirably explained the construction of the instruments made and improved upon by Dr Goring and himself, together with the history of the doublet, jewel, reflecting, and achromatic microscopes, the methods of testing and using the same, with the descriptions of many interesting objects observed by them. These works, which were the first of the kind published in England, have long since obtained a well-deserved reputation."—Pp. 44, 45.

We have hitherto mentioned only a few of the foreign artists who have contributed to the results now recorded.

Among these, the first place is assuredly due to Frauenhofer, a skilful practical optician at the small town of Benedict-Baiern, near Munich. This artist, who was born in 1787 and died in 1826, is well known among philosophers and physical inquirers for his valuable discoveries regarding the solar prismatic spectrum and its component colours. He had bestowed infinite pains on the preparation of microscopic lenses, which might be perfectly achromatic; and probably he was the earliest artist in Germany who accomplished that object. The first date, however, of the achromatic lenses of Frauenhofer, is not accurately known; and all that we know is, that he had made them several years previous to his death in 1826. M. Chevalier places it in 1816; we know not on what authority.

It would be wrong to omit the Berlin artists, Pistor and Schiek, a firm now, we believe, confined to the latter name; and from which establishment issued, at an early period, many microscopes on the improved principle. It was chiefly by instruments made by Pistor and Schiek, and, we believe, not of very high powers, that Ehrenberg made most of his discoveries in the structure of the brain, and in the history of infusory animalcules; and it is chiefly by their microscopes that such observers as Muller, Henle, and others of the German anatomists, have carried on their researches. At the present time good achromatic microscopes are to be got in almost every considerable town in Prussia, the Rhenish Provinces, the Palatinate, and Bavaria.

Chevalier is well known in Paris, not only as a maker of excellent microscopes, but as the author of a learned and intelligent treatise,* which deserves to be studied by all who are desirous to know the successive steps, by which this instrument has been carried to its present state of perfection, as well as by those who wish to understand the uses of the instrument.

Oberhauser is a German instrument maker settled in Paris, who has produced numerous microscopes of the most perfect and extraordinary properties. Several of these which it has happened to us to see have appeared to be fit for all that a correct and powerful microscope can be expected to accomplish.

Of the instrument, the history of which in the preceding sketch we have attempted to trace, it is important to understand the construction and the uses. The duty of explaining these has, we have already stated, been performed by M. Chevalier. Among English treatises previous to the publication of the present work by Mr Quekett, there existed the useful performance of Mr Pritchard, or rather, —for it was a joint production,—the Microscopical Essays of Dr Goring and Mr Pritchard, the article on the microscope by Sir David Brewster in the Encyclopædia Britannica, and the article on the same subject by Mr Ross in the Penny Cyclopædia. Of these three treatises it is difficult to say which is best; and after having been long in the habit of studying the essays of Dr Goring and Mr Pritchard, and Sir David Brewster's treatise, we are inclined to say that both are almost indispensable to the microscopical observer, especially at first.

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* Des Microscopes et de leur Usages. Paris.

The treatise of Mr Quekett is much more comprehensive and minute than either; and though it may not supersede entirely either of these two useful performances, it will certainly be quite indispensable to the microscopical student. The work is written by one well acquainted with the subject, and whose experience cannot fail to render his directions valuable. It is intended not only for the anatomical student, but for the botanist, the entomologist, the naturalist,—in short, for every one who desires to study minute and complex structures by means of the microscope. It is, in one word, a most instructive treatise on the microscope, and an excellent guide to the microscopical observer, whether beginning or advanced.

The work of Mr Quekett consists of three parts or general divisions. The first embraces the mechanical arrangements, the history of the instrument, the principles of construction, the mode of construction, and the detailed description of the different forms of microscopes. The second part is devoted to the use of the microscope. And the third part treats of manipulation, and the details of the mechanical contrivances, which the microscopic observer ought to understand, in order to pursue his researches with facility and success.

The First Part is subdivided into a preliminary sketch and five chapters. In the preliminary essay, the author gives a history of the progressive advances in the art of making microscopes, and traces the history of the instrument from its most simple and rude form to that, in which it has become the complete compound achromatic microscope. Chapter first is then devoted to the explanation of the construction of the simple microscope; chapter second, to that of the compound microscope; chapter third, to an account of accessory instruments, or those to which the microscopic observer has recourse, in order to assist him in his researches; chapter fourth, to the lamp and its uses; and chapter fifth is on the magnifying powers used with simple and achromatic compound microscopes.

The Second Part is subdivided into seven Chapters, in which are considered successively the following subjects. After preliminary directions regarding the position of the instrument, the adjustment of the light, and the adjustment of the focus, comes the subject of Illumination of Objects; the Examination of Opake Objects; the Use of the Leiberkuhn and the Dark Stops; the Different Forms of the Micrometer, and the Methods of Using it;

the Methods of Estimating the Magnifying Powers of Single and Compound Microscopes; the Camera Lucida, and the Method of Using it with the Microscope; the Polarization of Light, and the Method of Using the Polarizing Apparatus and the Goniometer.

The Third Part, on Manipulation, which is the most extensive, is subdivided into twenty-one chapters. Of these, the first is devoted to the subject of Diamonds, their Forms and their Uses; the second, to the Art of Cutting Glass; the third, to the Methods of Cementing Cells with and without Heat; the fourth, to the Method of Mounting Objects in Fluid; the fifth, to Cements of Different Kinds; the sixth, to the subject of Preservative Fluids: the seventh, to the Method of Mounting Objects in Canada Balsam; the eighth, to the Method of Mounting Objects in the Dry way; and, the ninth, to the Art of Mounting Opake Objects.

Another class of manipulations then follows. The tenth chapter is devoted to the subject of Making Thin Sections of Bone and Teeth; the eleventh, to that of Sections of Shell and other Hard Substances; and, the twelfth, to that of Making Sections of Wood, Horn, Hairs, and similar substances.

A third division follows, in the thirteenth chapter, on the Dissection and Preparation of Vegetable and Animal Structures, and the Instruments and Contrivances requisite for so doing; the fourteenth, on the Method of Dissecting Vegetable and Animal Tissues; the fifteenth, on Methods of Exhibiting Objects of Interest in Animal Physiology,—for instance, the Circulation of the Blood; the sixteenth, on the Circulation in Plants, and the Methods of Viewing it in Different Vegetable Structures; and, the seventeenth, on the Method of Procuring and Examining the Different Infusory Animalcules and the Fossil Infusoria.

Then follows a long but instructive chapter, the eighteenth, presenting the classification of the most important microscopical objects in the vegetable world, the animal world, and among mineral bodies. The nineteenth chapter is devoted to the Methods of Examining Morbid Structures, and the Rules and Cautions to be observed.

The twentieth chapter is devoted to the important subject of test objects, and the methods of examining them; and in the twenty-first are given various instructive hints and directions on the general management of the microscope, and cleaning its parts, and on microscopic preparations. In an appendix is contained an account of Mr Legg's improved stage,—an instrument for cutting circular covers of thin glass; and an apparatus for collecting infusory animalcules.

The work is illustrated with plates representing the best forms of microscope, and a great number of beautifully-executed woodcuts.

On one subject only we observe no information. We refer to the matter of microscopic illusions, or rather optical and other illusions liable to take place in the use of the microscope. On this subject, Henle, the professor of anatomy at Heidelberg, has given in his General Anatomy a most instructive chapter,* or rather a series of observations and cautions, of which we have long intended to give an abstract. The subject is one which the history of microscopical anatomy shows to be most important in reference to the use of the instrument in examining animal and vegetable structures, both in the healthy and in the morbid state. As, however, it is impossible at present to enter on the subject as treated by the German anatomist, it may not be out of place or season to subjoin, from the discourse of Mr Thomas Bell, some observations, the truth of which is confirmed by the results of daily experience. The observations are not directly upon illusions, but upon the discordant results which prevail among different observers, and which may probably be traced to illusions and to other causes.

> "And now," says Mr Bell, "I may ask, with what impressions do we review the brief sketch which I have attempted to lay before you, of some of the discoveries and investigations in physiological knowledge by means of the microscope?—what are the feelings and convictions which arise in our minds, on the mass of facts and assertions, as to the degree of certainty or of credibility on the one hand, and on the other, their bearing on the more general and important theories of physiology, as well as on the nature and treatment of disease? It is not without much reluctance that I am compelled to confess, that on my own mind, the result of the inquiry, however dazzling, is not wholly satisfactory. We have been contemplating a multitude of discoveries; things seen that had not previously been dreamed of; structures developed of the most admirable complexity; tissues unravelled; motions and functions in some cases ascertained, in many suggested, in still more asserted; but, notwithstanding the acknowledged importance of many of these facts, the question may be put, with the certainty of at least an unsatisfactory reply, Where are the laws which these discoveries have established? where the grand general views of physiological science which they have developed? and especially, where the practical foundations for the cure of disease which they have afforded? Is it not true,—and here I would again acknowledge with gratitude some honourable exceptions,—but on the whole, is it not true that, even where they remain undisputed, they have, in numerous instances, constituted

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* Allgemeine Anatomie. Lehre von der Mischungs-und Formbes tandtheilen des Menschlichen Körper. Von J. Henle. Leipzig, 1841. 8vo. Gebrauch des Mikroskops, seite 134. Optische Tauschungen, seite 138, 139, u. s. w.

brilliant and beautiful, but isolated facts, having but slight bearing upon the great ends and objects of practical research?

"And this is especially the case, with respect to those which relate to the illustration of pathological anatomy. Have the investigations, for instance, into the ultimate structure of the liver, given us one ray of light on the obscure, but well known and certain action of mercury on its secretion? Have the beautiful discoveries in the minute organization of the kidneys afforded any new grounds for the treatment of the too fatal diseases of those important glands? Have we acquired by this means any certain insight into the changes which take place in the different tissues during the progress of disease, or the characters by which may at once be detected the malignant or curable nature of tumours? Have we ascertained whether any, and what, appreciable characteristic change takes place in the passage from the curable to the malignant condition of a diseased organ? Are we yet in possession of the alterations which are produced in the different secretions by morbid action, or have we discovered whether such changes can be brought to bear upon a more certain knowledge of the condition of the organ in which they may be elaborated, and consequently upon the rational treatment of their diseases? These are a few of the questions which naturally arise in our minds, upon a calm and practical review of the results of microscopical research; and I adduce them, not so much for the puspose of showing how little has been done, as to excite attention to how much we may yet hope to see accomplished.

"But if the actual deficiencies in knowledge, compared with what might have been anticipated from the facilities for observation and the enthusiasm of observers, may excite some disappointment, there are other and far more serious defects in the results themselves, upon which, whilst it is painful to dwell, it might be useful to offer a few brief remarks. I allude, principally, to the obvious discrepancy which exists in the representations of the same structure; the confidence and boldness with which not only different, but opposite views are maintained and defended; and the manifest pre-occupation of mind, which we often see evinced by persons, who, instead of waiting to be taught by nature, and humbly submitting their judgment to their experience, strain what they see to the support of a preconceived notion, and then endeavour to bring others to fancy the same erroneous facts, and this, too, without any intention to deceive either themselves or others. Many of these errors arise from the inherent and unavoidable defects in the means of observation. Every one who has given himself devotedly to the study of the minute structures and forms, which require for their examination the higher powers of the microscope, will readily acknowledge, as they must have often experienced, not only the difficulty of obtaining a true and definite figure of the object with a power of 500 or 800 diameters, but the very strange changes which take place, according to the direction in which the light is thrown, or some scarcely appreciable differences in the condition of the object itself. I do not hesitate to affirm that, in the examination of transparent and soft structures, of such a degree of minuteness as to require so high a power to investigate them, a defined and certain figure can scarcely be obtained; and yet we see in the professed representations of such objects, as defined and sharp an outline, and as perfect a detail of light and shadow, as if the body had been carved in wood, and drawn from observation by the naked eye."—Pp. 19–22.

On these remarks it is unnecessary to offer one single word, either in the name of comment or illustration. Their force we cannot presume to increase. Their justice it is impossible to deny; and the most prudent and delicate course is, we conceive, respectfully to direct to them the attention and serious consideration of microscopical observers. It is such proceedings as those referred to by Mr Bell, that tend to create enemies to the use of the microscope, and prevent rational observers from reposing in its results that confidence to which they would otherwise be entitled. These injudicious friends are doing all they can to bring their favourite instrument into discredit; and if they would only reflect on the ridicule, as well as the distrust, which followed the observations of such men as the second Monro, Fontana, and Mascagni, and the globular objects to which Milne-Edwards, and afterwards Frederick Arnold, solicited the attention of Histologists, they would retain their observations for years rather than bring them forward in that state, in which at the best they can hope for only temporary attention.

"It is true, there are many exceptions, honourable to science and to truth, where the drawings are fair representations of what has been observed, and which are therefore characterised by all the dimness and uncertainty which belong to the objects themselves. But in the cases on which I have taken upon me to animadvert, the figures will serve well as diagrams to illustrate the author's own views and opinions, but are worse than useless as representations of nature. And not only are observers of this class so prone to see what they fancy, rather than to reason upon what they actually see, but they are not satisfied until they have persuaded others to sanction the same views; and thus the object, like Polonius' cloud, may be very like a whale, or it may be backed like a weasel; and the eyes are made the very fools of the fancy. The result is such as might be anticipated. The next observer of the same facts declares all the views of his predecessor erroneous. He has, perhaps, seen the objects under some slightly different circumstances, or he has thrown the light differently, or he has employed a different chemical re-agent, to make that visible which, without such an aid, would have eluded his observations; or he has either some other prepossession, or has honestly come to the examination without any preconception at all; and thus we have, at the present moment, scarcely a single supposed ultimate structure in the animal body, that is not represented under two or more different appearances, and with a similar discrepancy in its physical characters, and each defended with equal confidence, or even with all the acerbity of controversy. I abstain from offering any examples of the faults which I could not avoid noticing; but I may adduce the remarkable contrast between the representations of certain structures, as exhibited by some of the continental physiologists, and the obviously truthful, and therefore less distinct and defined, views of the same structures, as given, for example, by Mr Gulliver and M. Siddall. I might also notice the effect produced upon the whole world of zoological and physiological science, by the marvellous and beautiful delineations of the intestinal canal, with its spiral contortions and its cæcal appendages, and the course of the food through its tube, as exhibited by Ehrenberg in the infusory animalcula, and ask who has within the last few years seen, or even fancied, a similarly distinct tube running through the bodies of these animals? And yet no one doubts the entire good faith of the discoverer of their internal structure, nor of those who, following him in their observations, fancied for a time that they could discern all the organs which he depicted; and this was the more confirmed by the fact, that one most important part of his representations is so indubitably correct, as to have given a great degree of plausibility to the rest.

" It were easy to multiply instances of this unintentional error, but it is unnecessary, and would appear invidious. But it is a matter of no small importance to consider how it may be avoided, and how we may obtain, in the first place, the greatest degree of certainty and correctness as to the real nature of the objects observed, and, in the next, the irrefragable accuracy and truth of the conclusions deduced from them, so that every theory which we found upon microscopic investigations being *totus, teres atque rotundus*, may be as certain and unassailable as any of the known and acknowledged laws of physical science.

" I have already observed, that it is in those objects to which I have been all along alluding, namely, the ultimate structures of the animal body, that the highest powers of the microscope are necessarily employed, and, consequently, that it is in such investigations that there is the greatest danger of obtaining unsatisfactory and uncertain results. I am not called upon to expatiate on any points connected with the manipulation of the instrument, the importance of apportioning the degree of power to the nature of the object, of varying and modifying the light, and many other matters which can only be fully appreciated or effectually managed, by those who have had considerable practical experience in the use of the microscope; yet, to show the absolute importance of careful attention to these minor points, it would be sufficient to examine the same object exhibited in the self-same instrument by different persons, one of whom shall be but little accustomed to microscopic investigations, and the other, like a Bowman, a Quekett, or a Bowerbank, acquainted with all the minutiæ of handling and arrangement, upon which so much of the certainty and truth of the observations depend

"But ready manipulation, and the habit of accurate observation, are not the only requisites for deducing all the advantage possible from these studies. There must be added,—and on this I would anxiously insist,—a habit of reasoning; a thorough acquaintance with the process of inductive philosophy, by which the value of facts may be weighed, and their true bearing upon general laws soundly appreciated. It was this power which gave to Malpighi the superiority over all his contemporaries, and of which we have some brilliant examples amongst the present labourers in this branch of science.

"And, above all, I would recommend the practical application of the microscope to pathological anatomy, with the view of obtaining more certain views of the nature of morbid structures, and, consequently, of rational, well-founded principles, both of diagnosis and of the treatment of disease; and I cannot better conclude this most imperfect attempt, than by adopting the language of a distinguished continental physiologist:—

"'The microscope,' says Gerber, 'now recognized as indispensable in general and pathological anatomy, ought also to take its place among the implements most needful to the practical physician. It seems impossible, indeed, to over-estimate the extent to which the science and art of medicine would be advantaged, were every well-informed practitioner to examine each morbid product he encountered, and to communicate the results of his inquiries along with a compendious history of the case.'"—Pp. 22–24.

If anything were calculated to give weight to these observations, it is the circumstance, that they proceed from one who is himself both an excellent anatomist and a good microscopical observer, to whom, further, the unsolicited opinions of his friends have assigned the first place in the Microscopical Society. Mr Bell is evidently well qualified to act as a cool and judicious adviser of his impetuous friends; and there is every reason to believe that they would derive nothing but advantage from his assistance.

The nature of this Journal permits us not to enter more into detail on this subject. But we must not conclude without recommending, in the strongest terms, to all microscopical inquirers, the treatise of Mr Quekett, and the dissertation of Mr Thomas Bell.

Art. II.—*Lectures on the Physical Phenomena of Living Beings.* By Carlo Matteucci, Professor in the University of Pisa. *Translated under the superintendence of* Jonathan Pereira, M.D. F.R.S., Vice-President of the Royal Medical and Chirurgical Society. 16mo. Pp. 436. London, 1847.

In last volume, when considering the history of the doctrine of a vital principle, the limited space rendered it impossible to advert to circumstances of some importance in estimating the weight of evidence in favour of and against the admission of that principle, and also, in some degree, supposing the principle admissible, of no small moment in specifying the conditions under which that principle is to be admitted, and circumscribing the limits within which it must be allowed to operate. The circumstances to which we refer may be understood from the following observations.

The advocates for the doctrine of a vital principle have always maintained the necessity of distinguishing accurately between physical and vital phenomena, and between laws merely physical and chemical, and those which are believed to be vital and physiological. The justice of this distinction we believe all rational observers and sound thinkers will admit. The difficulty consists in specifying, at least in organized and living bodies, the points at which the one set of phenomena stops and the other begins; the characters which show that in animal and vegetable bodies certain phenomena are under the operation of laws merely physical, while other phenomena are as surely regulated by that different body of laws to which the name of vital and physiological has been accorded.

Bichat was a strong advocate for the great influence of vital and physiological laws in animal bodies, and for a sort of hypothesis that in living bodies there is a constant struggle, as it were, between physiological and physical laws. It was his opinion that physical laws in living bodies were more or less counteracted by physiological laws; and that it was only when life ceased to animate the body, that physiological laws ceased to operate, and physical phenomena and processes advanced with uncontrolled power.

Of others, again, it was the doctrine that in organized bodies physical and chemical laws were ancillary or subservient to vital laws; that the latter made the former act in the mode in which they were to be useful, and prevented them from being hurtful; and that it was only under certain peculiar circumstances that physical and chemical laws operated with such force as to show that they were no longer obedient to their ordinary superior. Thus in the healthy state the blood is moved in opposition to gravity. Acid substances taken into the system do not within it present acid phenomena and reactions. It has often been maintained by different physiological speculators that secretion consists in a process of filtration or separation of certain fluid parts from the blood, as that fluid was necessarily receiving fresh supplies from without. Such a doctrine, however, is directly contradicted by several decisive facts. Independent of the circumstance that each secreted fluid contains some peculiar matter characteristic of itself, and no other secreted product, one order of secreted fluids is more or less acid, and the other is more or less alkaline; while the blood, at least the serum, is always decidedly alkaline. The cutaneous transpiration, the gastric juice, the urine, are all acid in different degrees. The saliva, the bile, the pancreatic fluid, the fluid of the follicles, cutaneous and mucous, are all more or less alkaline. The question comes to be suggested, by what agency are these different fluids secreted with these characteristic acid and alkaline qualities? It is manifest that it is not mechanical filtration; for the fluids are too dissimilar for this. Neither can it be mere chemical action; for if such were the fact, all would be acid, or all alkaline, or all neutral. The chemical action is in this instance subordinate to some other action, whether it be called vital, organic, glandular, or physiological. To consider the question now stated, and if possible to solve the problem, or explain why it is insoluble, is the object of the inquirer who would present just views of the relative position of physical and physiological phenomena in living bodies.

In the year 1824, Dutrochet published his Anatomical and Physiological Researches on the Intimate Structure of Animals and Plants, and on the Mobility. In 1826, appeared, by the same inquirer, a Treatise, entitled the Immediate Agent of the Vital Movement, discovered in its nature and in its mode of operation in Vegetables and in Animals. And in 1828, the same writer published a third Treatise, containing New Researches on Endosmosis and Exosmosis, followed by the experimental application of these Physical Actions, to determine the Problem of Vegetable Irritability, and to discover the cause of the ascent of the Branches, and the descent of the Roots. Of these performances, an ample account was given in 1829, in the thirty-first volume of this Journal, and it is unnecessary now to recur to their contents. It is sufficient to observe that the researches of Dutrochet tended strongly and directly to give birth to new views on the nature of many of the phenomena taking place in organised bodies, and if not to limit considerably the influence and operation of what was considered the Vital Principle, certainly to diminish much the number of phenomena, and processes referred to the operation of that principle. The researches of Dutrochet showed, in short, that a most extensive class of minute movements in the vessels of animal and vegetable bodies, movements by which the fluids from without were moved inwards to various parts in these bodies, and fluids contained within these vessels were moved sometimes in one direction, sometimes in another, were all under the operation, not of vital action, but of mere physical principles.

Subsequently to the period now mentioned, namely, in 1837 and 1838, Magendie published his Lessons or Lectures on the

Physical Phenomena of Life and Vital Actions, and contributed' with the author already mentioned, to extend the domain of physical actions, and circumscribe somewhat that of vital actions, in the bodies of living organised beings.*

The researches of a third inquirer, namely, Poiseuille, some of which appeared also in this Journal, have tended to the same result. In short, it must be allowed that the circumstances now mentioned,—and others might be added,—have tended to make physiological inquirers look with greatly less confidence on the doctrine of a vital principle, and to believe that the actions and processes which have been ascribed to the operation of that principle, are much less numerous than has been heretofore thought. On the merits of this revolution in physiological doctrine, we profess to give no opinion. We are the historians, rather than the judges, of the doctrines of the day. It is important, certainly, to know the truth, to be satisfied as to the facts, whatever these may be. And the first step to appreciate and estimate the justice of doctrines, is to know the grounds on which they are founded.

Among those who have endeavoured to place on a just foundation the physical phenomena which are observed to take place in living bodies, no one probably is entitled to greater attention than Carlo Matteucci, Professor of Physics and Physiology in the University of Pisa. He has for a long series of years devoted great study to the phenomena of electricity and electro-magnetism; few are more intimately acquainted with physical science in general; and perhaps scarcely one person can be found who unites so accurate and extensive knowledge of physics and chemistry with an equally correct knowledge of physiology. In the year 1844, Professor Matteucci was appointed by the government of Tuscany to deliver, in the University of Pisa, a course of lectures on the physical phenomena taking place in living beings. These lectures were subsequently published first in the Italian and afterwards in the French language. They have been almost universally received in the most favourable manner, both from the high reputation of the author, and from the intrinsic merits of their contents. They constitute, in truth, the most judicious and the most instructive attempt to place in their true relation to each other the phenomena called physical, and those allowed to be vital, as they are observed to take place in organized bodies.

The work consists of twenty lectures, of which the first is introductory, and the other nineteen are devoted to the following subjects in successive order:—2. Molecular Attraction; Capillarity; Imbibition. 3. Endosmosis. 4. Absorption in Animals and Vegetables. 5. Digestion. 6. Respiration; Gaseous Endosmosis. 7. Hæmatosis; Nutrition; Animal Heat. 8. Phosphorescence of Organized Beings. 9. The Muscular Electrical Current. 10. Electrical Fishes; Proper Current of the Frog. 11. Physiological Action of Gravity; Light and Colour. 12 and 13. Physiological Action of the Electrical Current. 14 and 15. Nervous Force. 16. Muscular Contraction; Animal Mechanics. 17. Circulation of the Blood. 18. Vocal Apparatus; Voice. 19. Hearing. 20. Vision.

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* Leçons sur les Phenomenes Physiques de la Vie. Quatres tomes. 8vo. Paris, 1837-1838.

In the first lecture, after a review of the properties common to matter and organized bodies, the general question is stated in the following manner:—

"*Phenomena of Living Beings.*—From these considerations are we to conclude that all the phenomena of living beings are explicable, by the general properties which belong to them in common with all the bodies of nature, and by the sole action of the great physical forces, caloric, light, electricity, and attraction? Such an inference would be as far from the truth as the conclusion of those who have denied and still deny these general properties to living beings, and who regard them as entirely beyond the influence of physical agents.

"Examine those phenomena of living bodies, which, if I may be permitted so to call them, are *the most physical, the most chemical*, and you will find considerable differences in the mode of action of physical and chemical agents in the organism,—differences which are inexplicable in the present state of our knowledge of the laws governing these forces. Does not the phenomenon of vision itself, which may be termed a purely physical phenomenon, present peculiarities which remain up to the present moment unexplained? If the latest discoveries in science enable us to account for the distinctness of vision at all distances, and the absence of colour on the edges of the image, how, by the aid of physical laws, can we explain the perception of a single object in its natural position, by a double and inverted image? What could we not say of hearing and the voice, which are simply effects of some particular vibrations of the air, propagated by solids, according to the general laws of acoustics? To questions such as these, science can give no completely satisfactory answer.

"*Organisation.*—The chemical action of light, which decomposes carbonic acid, carries the carbon under the form of new combinations into the interior of vegetables, disengages the oxygen, and thus produces what the most powerful chemical affinities cannot accomplish, is certainly different from that which decomposes some oxides and metallic chlorides, an effect for the production of which the feeblest chemical actions are sufficient. Apply an electric current to the nerves of a living animal, and the peculiarity of the resulting phenomena will prove to you the immense difference which exists between the effects of the great forces of nature, according as the body in which they occur is living and organised, or inorganic and dead.

"What, then is the cause of these extraordinary differences in the modes of action of physical agents on living beings and on other bodies of nature? Here is a primary question of the highest importance, and one to which the existing state of our knowledge furnishes no satisfactory reply. But let us not, on that account, abandon the analogies which the physical sciences offer us; a ray of light which penetrates a piece of glass or a body of water, in an oblique direction, deviates from the straight line, whilst, on the contrary, if it fall on a crystal of carbonate of lime (calcareous spar), it is split into two other rays, each of which deviates from the direction of the primitive ray, but in unequal degrees. The cause of the difference of these phenomena resides in the difference of physical structure existing between glass and crystallised calcareous carbonate, and perhaps also in the different chemical nature of their molecules. These modifications of the luminous ray, however, arise more from diversity of structure, or the peculiar arrangement of the molecules, than from differences of chemical composition. Indeed, we know that glass acts differently upon rays of light, according as it is more or less compressed in different directions, without its chemical composition undergoing any change.

"Who could confound an organised being with an inorganic body? In these groups of closed vesicles, of different dimensions, united and disposed in an irregular manner, there is assuredly *something* essentially different from a mass of polyhedral particles, composing a crystal. To say, with some micrographers, that organisation is crystallisation effected in a liquid which the first formed crystals imbibe, is equivalent to admitting that the structure of a stalactite is identical with that of the lungs and the liver. Molecules, composed of at least three elements, into each of which a great number of elementary atoms enter, must necessarily form chemical systems, whose affinities differ from those which are possessed by molecules chiefly composed of two elements, and in which the number of elementary atoms is smaller. And if the general chemical actions, by showing us that combinations become weaker in proportion as the number of the elementary atoms increases, are sufficient to explain the tendency of organic bodies to resolve themselves into more simple combinations; if chemistry furnishes us with many instances of this same tendency in some inorganic compounds, whose composition has many analogies with that of organic bodies, it is not, therefore, to be inferred that the laws of inorganic chemistry are sufficient to give us a complete explanation of all the chemical phenomena of life. We must then conclude that organisation and the molecular structure of living beings effect important modifications in the action of physical and chemical agents.

"*Actions of Contact or Catalysis.*—We must, however, omit to add that each successive day increases the number of a particular class of chemical phenomena whose explanation is impossible by the ordinary laws of affinity only; I refer now to actions of *contact* or *catalysis*. In the greater number of these we find that a substance, usually in very small quantity, excites in other compounds, without itself undergoing any modification, considerable changes either of chemical composition or of physical properties. To this category of phenomena belong the various kinds of fermentation. We shall find that the number of catalytic actions in living beings is immense. Admitted. We can also produce them in our laboratories; they are of the same nature as those which platinum black effects on a mixture of hydrogen and oxygen, and which finely-divided silver produces on peroxide of hydrogen.

" *Cell-life.*—I ought here, also, to mention a fact of importance which I shall have occasion hereafter to speak of more fully. The *cell* is certainly the elementary organ, the molecule of organic bodies. We can now, by the aid of the phenomenon of endosmose alone, effected entirely under the dominion of physical forces, explain the mechanism of cell-life; we can tell how the materials necessary for nutrition are able to penetrate the cell, whilst others are eliminated. We shall go over together an extensive series of physiological facts, of which endosmose has furnished the explanation.

" *Vital Phenomena not completely explicable.*—We may also add, and we intend to demonstrate the fact to you, that light, heat, and electricity, are produced in living beings by the same physico-chemical actions as those which take place in inorganic bodies, and that they offer the same results. But, with the aid of this knowledge and of these analogies, dare we hope to obtain a complete explanation of all the phenomena of living beings? For the present, at least, this would be a vain hope.

" Open an animal, examine its kidneys and its liver, and then ask yourselves by what physical force you can explain how the blood, which is carried to an organ, forms bile and urine. Can we, by having recourse to chemical affinities, however modified and aided by the peculiar structure of the organs, and even also by the actions of contact,—can we, I will not say comprehend, but even obtain a glimpse of the way in which the various organs effect the separation and transformation of the constituent parts of the blood, in which all the organic elements are mixed, partly suspended, partly dissolved, and of which they have need, in order to repair their continual losses? What can we say of the functions of the nerves and generation?

" *Conclusions.*—We conclude, therefore:—

" *1st,* That living beings have the general properties of all the bodies of nature; that these properties are influential in the production of the phenomena proper to them, and that, consequently, we must not neglect or disregard them when we attempt to explain these phenomena.

" *2dly,* That the great physical agents, caloric, light, electricity, and molecular attraction, act on living beings as well as on all the bodies of nature, and that their action must necessarily be influential in the production of the functions peculiar to these beings.

" *3dly,* That these forces, when acting on organised matter, sometimes have their general mode of action modified, and that this difference is owing to a diversity in the structure and chemical composition of organised bodies.

"*4thly,* That there are also in living beings phenomena which we call vital; that these are numerous, and of the highest importance; and that, in the present state of science, we are unable to explain how their production can be influenced by physical agents, though the action of these be modified by the organism. This is the reason that we have a study,—a science whose object is the physico-chemical phenomena of living bodies; as there is one for experimental physiology. The intimate and necessary connection is found in the third class of facts which we distinguished. Organisation modifies the action of physical agents, and the study of these modifications requires the co-operation of physics and experimental physiology. Do not forget that we have formed a fourth class of phenomena of living beings, which we have called vital. I term them vital phenomena, not vital forces, and indeed the difference is truly vital."—Pp. 6–12.

The following remarks will show in what light the author considers the usual language about the Vital Forces:—

"To speak of the vital forces, to give them a definition, to interpret phenomena by their aid, and yet to be ignorant of the laws which govern them, is doing nothing, or rather it is doing what is worse than nothing. It is to attempt an impossibility, it is to content the mind to no purpose, to stop the search after truth. To state that the liver separates the elements of the bile from the blood by means of the vital force, is merely to assert that the bile is formed in the liver. By thus varying the expression, a dangerous illusion is established.

"*Physico-chemical Phenomena.*—I believe that I have clearly shown the object we ought to aim at in studying the phenomena of living beings, which returns in its ultimate analysis to the examination of the physico-chemical phenomena of these bodies; of the modifications which organisation effects in the general action of physical agents; and, lastly, to the investigation of the laws at present empirical, of the purely vital phenomena.

"I hope I have succeeded in fully determining what are the limits within which we ought to confine ourselves in the vast domain of physiology, and what part of the subject we ought to study under the title of the *physico-chemical phenomena of living beings.* The generalities which I have now stated must be sufficient to prove the importance of understanding the functions of living beings."—P. 13.

In the third lecture, after treating of capillarity and molecular attraction, Professor Matteucci gives an account of the phenomena of imbibition, or that process by which fluids which are in contact with living bodies, are drawn from without into the interior of these bodies, and made to contribute to their nourishment and growth. This process is common to vegetables and animals, but is more general in the former. It is a process almost entirely mechanical. In solutions thickened with gum, with boiled starch, or with oil, it scarcely takes place in any degree; and it is also very feeble in concentrated saline solutions, and in all liquids holding in suspension very finely divided particles of solid matter. In the latter case it produces a kind of filtration. This phenomenon of imbibition may, in the case of solutions holding in suspension very finely divided molecules of solid matter, be serviceable in ascertaining the different properties of the blood, according to its density. In certain diseases, indeed, the density and viscidity of the blood are much diminished; and in such circumstances it is, according to Professor Matteucci, that serous infiltrations take place, as they do, he adds, for the same reasons, after profuse discharges of blood. He subsequently shows that alkohol, ether, water, and aqueous solutions, when introduced into the stomachs of living animals, disappear, though at unequal intervals of time, while oil remains there for a very long period.

It seems to us difficult here to say, how much or how little of the phenomena now specified are rightly referred to the process of imbibition alone. Some of the articles now mentioned are certainly taken up directly by the veins, whether this process be denominated imbibition or absorption. Some, also, are not readily taken up, because they are not digested, or presented in that form in which they can be taken up. Thus oil and all oleaginous matters, it has been long known, are not directly taken up or imbibed in the stomach, because they require previously to undergo the digestive changes, which do not take place until they have reached the duodenum, where the affusion of bile and pancreatic juice converts them into a sort of emulsion. For it is little that the alkaline properties of the saliva which is swallowed can do for this purpose.

In animal bodies imbibition is most easily accomplished with the cellular tissue and the parenchyma of the lungs. But on the skin the epidermis forms a barrier almost insurmountable.

Imbibition is more active under high than under low temperatures; and with warm liquids than with those that are cold. He finds it the same, however, in air saturated with moisture and in perfectly dry air. In the vacuum of the air-pump, imbibition through sand is more rapid at first than under the ordinary pressure of the atmosphere.

On the mode of explaining the phenomena of ENDOSMOSIS, the following observations are given :—

" *Theory of Endosmose.*—Dutrochet has endeavoured to give an explanation of the phenomena of endosmose, and Poisson and Becquerel have proposed others. Thus, some ascribe endosmose to the action of an electric current developed by the contact of the two different liquids ;—a current which will produce the passage of the water through the membrane, from the positive to the negative pole, as in the well-known experiment of Porret. But to render this explanation probable, it would be necessary to prove that the contact of water with alcohol, solution of sugar, &c. developes electricity; which is not the case. Poisson supposed that the least dense liquid entered the capillary tubes of the membrane, and that this capillary thread, drawn down by the pure water, and up by the denser liquid, must be elevated in virtue of molecular attraction. But this explanation becomes inadmissible when we consider that alcohol, which is lighter than water, produces endosmose, and that certain calcareous and siliceous stones, placed under the same conditions as membranes and plates of clay, do not give rise to the same effect.

"Up to the present time we have not any satisfactory theory of endosmose; but we know that the general conditions of the phenomenon are as follow:—

"*1st,* That the two liquids should have an affinity for the interposed membrane.

"*2dly,* That the two liquids should have an affinity for each other, and be miscible.

"If one of these conditions be wanting, endosmose does not take place. Experiment proves that the current of endosmose is not produced by the least dense liquid, nor by the most viscid one, nor by that which is endowed with the greatest force of ascent in capillary tubes. The current is in general determined by the liquid which has the greatest affinity for the interposed substance, and by which it is imbibed with the greatest rapidity. In fact, it is evident that the membrane imbibes the two liquids unequally; and that the one which is imbibed with the greatest facility, ought to mix with, and augment the volume of, the other.

"*Endosmose in Living Beings.*—What we have here stated must be sufficient to convince you that this phenomenon is perhaps one of the most important physical facts applicable to the functions of living beings. Microscopic observation has now put beyond doubt that, in all tissues, whether vegetable or animal, and in those liquids which are produced by the alteration of organised and living beings, there are constantly found, at a certain epoch, microscopic corpuscles, which have a peculiar and characteristic form, and are called elementary or primitive cells. These bodies consist of an exceedingly delicate membrane, which has a spherical form, encloses a liquid, and has on its inner side a small organised body, called the *nucleus* or *cyto-blast.* The cells float at first in a liquid, which Schwann has named *cyto-blastema,* and they ultimately become included in, and almost confounded with it, when this liquid acquires a greater or less density. In different tissues, the elementary cells are more or less closely approximated to each other; the *cyto-blastema* or intercellular substance being invariably the bond of union between them. The life of the elementary cells certainly plays the most essential part in the development and preservation of the tissues of living bodies; and, since these cells are found under conditions favourable to endosmose, we have no reason for refusing to admit its existence. A vesicle filled with a liquid, and placed in the midst of another liquid, may act on the outer one, receive the surrounding liquor, and reject the one it had previously contained, by operating in a manner analogous to endosmose."—Pp. 38–40.

On the condition of the membranes favourable or unfavourable to the process of endosmosis, it is important to advert to certain circumstances as of recency, dryness, or moisture, and investing textures.

"Some differences, as obvious as those observed when using fresh membranes, disappear entirely, or nearly so, when we employ membranes dried or altered by a more or less advanced state of putrefaction. We have not much varied the experiments proper for determining the influence of the condition of dessication and putrid alteration of membranes, and we intend hereafter to resume our examination of this subject. It is, however, certain, that when employing the ordinary liquids, and interposing between them and pure water the dried bladders of a pig and an ox, which have been moistened before the experiment, so as to enable them to be applied to the endosmometer, that there is either no difference in the elevation of the liquids in the two tubes, even after several hours, whatever be the position of the surfaces of the membrane; or a very slight difference in the instrument, in which the internal surface of the bladder is towards the interior of the endosmometer, sometimes in the other one. When employing bladders which have been left for some hours in water, we occasionally observe a certain regularity of effects, as with fresh bladders; but if we employ them very wet, after they have soaked a whole night in water, we observe no elevation in the liquids of the endosmometers, or the elevation, which is always slight, is equal in the two tubes. We can, in some cases, explain the anomalies presented by the bladders in this state. Thus any one can perceive on a wet bladder, that the muscular fibres are swollen, in proportion to the time the bladder has been in water. These muscular fasciculi acquire thus a certain thickness, they approximate to one another, and acquire, in some degree, a condition analogous to that of freshness. But we have several times seen that endosmose does not take place with bladders, gizzards, and fresh stomachs, from which the muscular laminæ had not been removed. If the bladder be slightly moistened, the muscular bundles are, it is true, a little more expanded, but nevertheless there exist between them interstices through which endosmose certainly takes place: the inequality of the interstices, however, even in two symmetrical portions of the same bladder, must produce vague and uncertain results.

"We have only employed the gizzard of the fowl in a more or less altered state, in order to determine what influence putrefaction has over the phenomenon of endosmose; a great uncertainty exists with respect to the results furnished by the gizzard in this state. Sometimes, indeed, the liquid did not pass at all, sometimes it had an equal elevation in both instruments. Whatever were the liquids employed, or the position of the membrane, endosmose was energetic, but sometimes in one direction, and sometimes in the other; and occasionally, indeed, it was depressed to the level of the liquids in both instruments. In speaking elsewhere of that which we observed in making use of the skin and gastric mucous membrane of certain animals, we have remarked that the phenomena of endosmose vary according as we employ these membranes immediately after death, or some hours subsequently. All these facts demonstrate clearly the intimate relation which exists between the phenomenon of endosmose and the physiological condition of the membranes.

"The phenomenon of endosmose, like every thing going on in organised tissues, is devoid of that constancy and regularity observed in physical phenomena elsewhere. To this variable and accidental organic condition of the fresh membranes must be ascribed the singular fact, that while in certain cases we obtain an elevation of perhaps 80 millimetres, yet sometimes with the same liquid, the same membrane, and in the same relative position, the rise does not exceed 10 millimetres. We must also ascribe to a constant anatomico-physiological condition, connected with the function of the same membrane, that constant difference of elevation in the two different positions of the membrane, whatever this difference may in other respects be. It is important to study the phenomenon with the view of recognising the accidental circumstances which cause the variation of endosmose through the fresh membranes; as, for example, the privation of nourishment in relation to the stomach, the administration of certain substances to the animal before killing it, &c. With this object we made one comparative experiment only, which induces us to believe that endosmose through the skin of the eel is most energetic when the skin has been removed after the animal has been for some time out of the water.

"*Conclusions.*—The novelty and importance of the results we have obtained, must be my excuse for relating them in this extended form. The general conclusions which we have deduced from them are as follows:—

"*1st*, The membrane interposed between the two liquids is very actively concerned, according to its nature, in the intensity and direction of the endosmotic current.

"*2dly*, There is, in general, for each membrane, a certain position in which endosmose is most intense; and the cases are very rare in which, with fresh membrane, endosmose takes place equally, whatever be the relative position of the membrane to the two liquids.

"*3dly*, The direction which is most favourable to endosmose through skins, is usually, from the internal to the external surface, with the exception of the skin of the frog, in which endosmose, in the single case of water and alcohol, is promoted from the external to the internal surface.

"*4thly*, The direction favourable to endosmose through stomachs and urinary bladders varies, with different liquids, much more than that through skins.

5thly, The phenomenon of endosmose is intimately connected with the physiological condition of the membranes.

"*6thly,* With membranes, dried or altered by putrefaction, either we do not observe the usual difference arising from the position of their surfaces, or endosmose no longer takes place."—Pp. 62-66.

"*Endosmose of Cells.*—A cell is the elementary organ of all animal and vegetable tissues, and cell-life involves an act of endosmose: this shows how much the phenomenon of endosmose requires to be more completely studied, in order that we may be enabled to make of it all the applications of which it is susceptible.

"*Endosmotic Action of Purgatives.*—I cannot conclude this lecture without referring to the recent experiments of Poiseuille, made with the view of explaining by endosmose the purgative action of certain substances. He found that there was endosmose through animal tissues, from the serum of the blood to Seidlitz water, and to solutions of sulphate of soda and common salt. Now this is precisely what happens when we use these medicines internally: the rejected excrements contain an abundant quantity of albumen. In this case we must admit, that endosmose takes place through the capillary vessels of the intestine, from the serum of the blood to the saline solution introduced into the alimentary canal."—P. 73.

"These physical phenomena, which, although they do not explain all the peculiarities of the digestion of fatty substances, nevertheless contribute to render them less obscure. The chyliferous vessels, which terminate in closed or blind extremities (*en cul-de-sac*), and are enveloped by intestinal mucus, are, especially in a fasting animal, filled with an alkaline liquid, very analogous to lymph. After digestion, particularly when the animal has been fed on fatty substances, the liquid of the chyliferous vessels differs from what it was previously, merely by the addition of fatty corpuscles, which give it the milky appearance. It is natural to suppose, that this chemical affinity, which produces the milky liquid in the mixture of the alkaline solution and oil, is also exerted through the membrane of the chyliferous vessels, which certainly imbibes as much of the alkaline solution as of the milky liquid, formed by the action of the alkali on the fatty bodies.

"The phenomena of endosmose, of which I have spoken, may also be admitted, with great probability, as one of the causes which produce absorption by the chyliferous vessels. It is certain, that absorption could not take place, if the inner sides of the intestines were not bathed with some liquid, for which the fatty bodies had some affinity.

"It is easy to demonstrate, by experiments, that the alkaline condition of the intestinal coats favours this absorption. Fill two funnels with sand, equally shaken down in each. Pour into one pure water, into the other an alkaline solution; when the liquids have filtered through, pour an equal quantity of oil on each filter. For several hours, the oil will remain upon the surface of the sand, which has been moistened with pure water; whilst in the other funnel, in which the sand has been moistened with the alkaline solution, the oil will rapidly disappear by imbibition.

" The neutral azotised substances which pass into the blood, after having been dissolved by the gastric juice, would rapidly destroy the neutral or slightly alkaline condition necessary to the preservation of the qualities of the blood; but the alkali of the chyle, of the lymph, of the bile, and of the pancreatic fluid, preserve the neutrality of it.

" *Origin of Cells.*—The chyle and the lymph hold in suspension a great number of small grains, which are from 1 to 2 thousandths of a line in diameter, and which appear to be formed of a fatty substance enveloped in a membrane, which is supposed to consist of a substance analogous to proteine. These same granulations exist in yolk of egg, in the milk, chyle, lymph, and in all the liquids exuded in pathological cases, or destined for new formations. These elementary granulations have been seen to unite and form a globule, a cell analogous to blood-cells; hence, they have been regarded as the morphological elements of all animal tissues.

" Recently, Donné observed, by injecting milk into the blood-vessels, that the globules of milk disappeared, after some time, by becoming covered with an albuminous layer like a bladder; that they then become reduced to the condition of white globules of the blood, which, finally, also disappeared, being probably transformed into red globules, Afterwards, all the blood re-acquired the appearance which it had before the milk had been injected into it.

" The organic element seems then to be reduced to a vesicle consisting of a layer of albumen collected together and organised around a *nucleus* formed principally of a fatty substance.

" I can bear testimony to an important experiment made by Ascherson: it consists in putting a fatty liquid in contact with albumen. This latter instantly coagulates, as you here see. If you mix them together, and put a drop of the mixture under the microscope, you will perceive a group of vesicles, each formed of a granule of oil, enveloped by an albuminous membrane, in some degree coagulated, and, it appears, like what the real adipose cells would do, on the stage of the microscope. We can see this still better, by putting on a plate of glass, a drop of oil, and one of albumen, and slowly bringing them in contact: it is curious to observe, by the microscope, the almost instantaneous formation of a very delicate and elastic membrane, which soon acquires numerous folds. Ascherson has proved that this formation, produced by albumen and oil, is really of a cellular nature. By adding a little water to a drop of this formation, he saw the cells swell up, and at the same time some smalls drops of oil escaped. By using diluted acetic acid instead of water, the cellules appeared to him to become so voluminous, that they burst. In oil, on the contrary, they became compressed and diminished in size.

" Evidently these facts, which nevertheless require to be varied and extended, belong to the phenomenon of endosmose, and cannot be comprehended without admitting the cellular formation. Here, then, is a physico-chemical operation, which may lead to the discovery of the formation of elementary granulations. Fatty substances, and combinations of proteine, are constantly introduced into the organism; they are met with in all animal tissues; the globules of fat, which pass into the chyliferous tubes, and are there found in an albuminous liquid, must soon become enveloped by analogous membranes; and ought, for this reason, to form vesicles resembling those which microscopic observation discovers in the chyle, the lymph, and the blood."—Pp. 112-116.

In Lecture X. the author enters on the difficult and important subject of the excitement and transmission of electrical currents by various animal tissues; especially the nervous chords and the muscular fibres.

"The proper current of the frog has, then, for the organic element, the leg only.

"Recently, by studying more attentively the proper current, I have satisfied myself that it is a phenomenon which appertains to all animals. Here is the enunciation of the fact. In every muscle endowed with life, in which the tendinous extremities are not equally disposed, there exists a current directed from the tendon to the muscle, in the interior of the latter. All animals have some muscles, in which one tendinous extremity is narrower than the other; and which, at one part, forms a kind of cord, and at the other becomes broader and ribbon-like. In the frog and many other animals, the gastrocnemius has this character; in birds, the pectoral muscle presents this arrangement, When we form a pile with these muscles, we find that a current circulates in the muscle, from the tendinous extremity to the muscular surface.

In arranging this pile we must carefully avoid exposing the internal part of the muscle, and we must especially place one element in contact with another, in such a manner that the tendinous extremity touches the surface of the muscle, and never its interior; indeed, the latter ought to be as far as possible from the tendon. Without this precaution there will be, in the circuit, the muscular current which, being directed from the interior to the surface, would have a direction precisely the reverse of that of the proper current. The existence of the proper current of the frog, in all animals in the way described, was found at the same time by M. Cima, by M. Boy-Raymount, at Berlin, and by myself.

"Having thus ascertained the conditions on which the proper current depends, I think that I may generalise its origin, and connect it with the muscular current. This community of origin is principally demonstrated by the identity of action which the different circumstances that modify the organism and life of animals, exercise upon the muscular current. In fact, whether the current be muscular or proper, the action exercised on it by heat, narcotics, sulphuretted hydrogen, and the degree of integrity of the nervous system is the same."—Pp. 226-227.

"The different methods of proceeding to ascertain the effects of various toxicological agents upon the excitability of the nerves to the passage of the electrical current may be reduced to two. One consists in ascertaining the number of elements necessary to excite contractions both in poisoned and in uninjured frogs; the other, and preferable method, is to compare the time required for the passage of a given current to destroy entirely the nervous excitability of a poisoned animal, and of another animal killed in the usual way.

"Animals which have perished in hydrogen, azote, carbonic acid, and chlorine, and are submitted to the passage of the electric current through their nerves, present no difference from other dead animals which have not been subjected to the action of these gases. But this is not the case with those killed by hydrocyanic acid, or by the repeated discharges of a large battery through the spinal marrow. In these cases the current furnished by one, or even many elements, applied upon the nerves of an animal, excites no contraction there, or if there be any they are very slight, and the passage of the current for a few seconds is sufficient to destroy them entirely. The muscles, however, when submitted to this same current, give very evident signs of contraction; thus proving what I have before mentioned, that the muscular fibre must possess the property of contracting, under the influence of the current, independently of the nerve.

"Lastly, if the animals on which we act by the electric current have died in sulphuretted hydrogen, we never obtain contractions unless very powerful currents are employed, and even then the contractions soon cease; and that whether we act on the nerves or on the muscles."—Pp. 255, 256.

"I cannot here give you all the details of the numerous experiments I have made with this instrument, for the purpose of determining the force of the contraction excited by the electric current in different cases. But here are the general conclusions at which I have arrived:—

"*1st,* The contraction excited by the electric current, transmitted along a nerve in the direction of its ramification, and which we call *direct*, is always more energetic than that which this same current produces when passing along the nerve in the opposite direction.

"*2dly,* The direct current weakens and rapidly destroys the excitability of a nerve; whilst the passage of the inverse current augments it within certain limits.

"*3dly,* To produce these effects, the action on the nerve, of the direct as well as of the inverse current, ought to be continued for a certain time, which will be longer in proportion as the excitability of the nerve is weaker.

"It is very easy to prove, by experiment, the most important of these conclusions; that is to say, that when the direct current traverses the lumbar nerve of a frog for twenty or thirty minutes, there are no further contractions, either when interrupting or closing the circuit: on the contrary, the contraction obtained by opening the circuit of the inverse current, after many hours' passage, scarcely differs from that which occurred at first, when the nerve was endowed with great excitability. This difference in the excitability of a nerve, according as it has been submitted to the passage of a direct or inverse current, can be observed whatever may have been the manner in which the nerve is stimulated. When we operate with the inverse current on a very excitable nerve, and one which has never before been submitted to the passage of the current, it is impossible to discover any difference between the contraction excited by the opening of the circuit of this current after the passage has continued for one second, and that which occurs after the passage has been continued for ten or twenty seconds. It does, however, exist: but to appreciate it, it is necessary to proceed more rapidly. If the passage of the inverse current be limited to a small fraction of a second, we find, on opening the circuit, a weaker contraction than that which is obtained after the current has circulated for one or more seconds. It is very easy to prove this, by closing the circuit by the aid of a wheel furnished with a metallic tooth, and to which we attach one of the wires of the pile during its rotation. When the nerve has lost a part of its excitability, we then readily perceive that the contraction manifested on opening the circuit increases proportionally to the time that the circuit has been closed. The greatest effect of the passage is obtained at the end of fifteen or twenty seconds. It is needless to say, that these effects do not continue to increase on a dead animal."—Pp. 262, 263.

" *Theory of the Action of the Current on the Nerves.*—With the knowledge of these facts, all of which I have recently established by a great number of experiments, I hope to be enabled to give a very simple theory of the action of the electric current on the nerves, and of the phenomena which it produces in animals.

" No experiment demonstrates that the electric current excites muscular contraction during its passage in the nerves. This passage only modifies the excitability of the nerve. Contraction is constantly produced by the effect of the electric discharge, properly so called, namely, by the neutralisation of the two opposite electrical conditions accumulated at the poles, and which give the spark. Every one knows, that when we close the circuit of a pile, as well as when we open it, there is a spark. Under precisely the same circumstances, the current always excites contractions. It is sufficient to have once seen the contractions of a frog excited by touching its nerve with the armatures of a Leyden bottle, which has already been discharged several times by a metallic arc, to be convinced how excessively feeble is the discharge capable of producing this effect. A bottle which, as we have stated, has been already discharged many times, can yet produce fifteen or twenty contractions in a frog.

" On the discharge of the bottle, we likewise see that the contraction of the limb, traversed by the inverse discharge, first ceases, whilst that which is provoked by the direct discharge continues.

" There is no difficulty, therefore, in understanding why, when the excitability of the nerve is lessened, the spark produced by the interruption of the direct current should excite no farther contractions. With the inverse current we obtain the contraction by the spark at the opening of the circuit, because, in the interval of its passage, the excitability of the nerve has increased. This augmentation disappears immediately the current ceases to act, and this is the reason why the spark no longer excites contractions when we subsequently again close the inverse circuit.

"With these notions we can also understand the fact of the voltaic alternatives. When a nerve has been for some time traversed by the direct current, and has lost its excitability, it no longer suffers contraction, notwithstanding that there may be a spark at the closure, and at the interruption of the current. The inverse current restores to the nerve a portion of its excitability, and its contraction reappears when we open the circuit. If from the action of the inverse current we return to that of the direct current, the contractions which are obtained during the short time that the nerve preserves the excitability acquired by the passage of the inverse current, will be more energetic, inasmuch as we have seen that the direct discharge produces on a nerve, endowed with a certain degree of excitability, a stronger contraction than the inverse one.

"*Effects on other Parts of the Nervous System.*—To complete this lecture, it only remains for me to speak of the effects which the electric current produces when applied to different parts of the brain, to the nerves of sensation, to the roots of the spinal nerves, and to the ganglionic nerves. I regret that a subject so important has not hitherto been properly investigated. We may say that everything yet remains to be done. The few words which I can say to you on the subject will prove it.

"*Effects on the Brain.*—I applied the conductors of a pile, formed of several elements, to the cerebral hemispheres, and to the cerebellum of the brains of a living animal; and I introduced the conductors into the very substance of these organs, without perceiving either convulsions or signs of pain. But by bringing the conductors in contact with the *tubercula quadrigemini*, the *crura cerebri*, and *medulla oblongata*, we obtain very violent convulsions throughout the body, and the animal gives signs of suffering.

"*Effects on the Spinal Marrow and the Roots of its Nerves.*—In conjunction with Longet, I examined the action of the electric current upon the roots of the spinal nerves, and on the fasciculi of the spinal marrow. The following are the results obtained. With the anterior roots, which are for motion, there was, as usual, in the first period, contractions produced both when we closed and when we interrupted the circuit, whatever was the direction of the current. In the second period of excitability, we obtained, by acting upon the anterior roots, the opposite effect to that which took place upon the mixed nerves; the inverse current excited contractions in the first moments of its passage, and none when it ceased; the direct current, on the contary, produced them when it was interrupted, and not when we closed it. It is unnecessary to add, that contractions were never produced when we acted upon the posterior roots, provided that we had divided the anterior ones. The anterior fasciculi of the spinal marrow offered the same phenomena as the corresponding roots. These differences appear to me of the very highest importance.

"I have recently found that a mixed nerve, after having been submitted to a great number of successive discharges, such as can be obtained with an electro-magnetic machine, presents, *for a certain time*, the phenomena of the anterior roots now described.

"This study, I repeat, will be of the highest importance for the physics of the nervous system; and the facts related lead us to assume that the differences obtained with different nerves are due rather to a difference of structure than to a different state of the nervous fluid.

"*Effects on the Nerves of Sensation.*—Let us now pass to the nerves of sensation. Magendie caused the current to pass through the optic nerve of a living animal without obtaining contractions or symptoms of pain. In operating on himself, by touching with the extremities of a pile formed by a single element, the ear and the eye, or the ear and the tongue, or the eye and the tongue, he obtained sensations of sound, flashes of light, and a peculiar taste. These effects could only depend on an action exercised by the current on the sensorial nerves of these organs, and not on the contractions excited in the muscles dependent on them. In fact, a very feeble current, insufficient to excite the slightest muscular movement, is capable of acting upon the senses. The peculiar taste cannot be attributed to the impression exercised upon the tongue by substances produced by the decomposition of the salts of the saliva effected by the pile; for a very feeble current, and one that is unable to cause this decomposition, is yet sufficiently strong to give rise to the electric sensation upon the tongue.

"*Effects on the Ganglionic Nerves.*—A few words, in the last place, on the action of the current on the nerves of the ganglionic system. For the little that we know on this subject, we are indebted to the celebrated Humboldt.

"When we transmit a current through the heart of a recently killed animal, a few instants after pulsations have ceased, we observe that this organ recovers its usual movements some time after the passage of the electric current, and that these movements are preserved for a certain time after the organ has been withdrawn from the action of the current.

"If, instead of waiting until the natural movements of the heart have entirely ceased, we transmit the current when they are merely becoming weaker, we then perceive that when the latter has acted upon the heart for some time, the movements become more frequent, and the augmented frequency continues for several seconds after the current has been interrupted.

"The same effects take place with the vermicular movement of the intestines, when these organs are subjected to the influence of the current.

"If we reflect on the importance which the ganglionic system possesses in the performance of the *organic functions* of animals, we can easily understand how very insufficient are the researches hitherto made on this subject.

" The difference of action exercised by the current upon the nerves of the life of *relation* and on those of *organic* life, is very marked. In the former, its effects are manifested only in the first and the last moments of its application ; in the latter, on the contrary, they are slow to appear, continue during the passage of the current, and even persist after it has been interrupted."—Pp. 264-270.

" *Therapeutical Use of the Current.*—I cannot conclude this lecture without stating some of the therapeutical applications made of the electric current, and founded upon the scientific principles, which I have made known to you.

" *In Paralysis.*—Abstraction being made of all purely theoretical ideas, and independently of all hypothesis of the nervous force, we may admit that, in certain cases of paralysis, the nerves undergo an alteration analogous to that which they would suffer if they had been subjected to a continued passage of the electric current. We have seen that, in order to restore to a nerve the excitability lost by the passage of this current, it is necessary to subject it to the action of the inverse current.

" I must add, in favour of the efficacy of the therapeutic use of this current, that a limb, although paralysed, constantly suffers some contractions when it is submitted, either to the passage of a current or to the action of electric discharges ; and these contractions favour the restoration of the functions of the muscles. Experiment confirms these ideas : divide the two sciatic nerves of a living frog, allow one of the two limbs to remain quiet for ten, fifteen, or twenty days, and submit the other, two or three times a day, to the action of the current. The latter will continue to contract, whilst the other will fail to give any contractions when the current is applied to it.

" I am anxious to state to you some rules which I consider as important in the application of the current to the treatment of paralysis. You should always commence by employing a very weak current. This precaution seems to me now more important than I formerly believed it to be, having seen one paralytic patient seized with true tetanic convulsions under the action of a current furnished by a single element. Take care never to continue the current for too long a period, especially if it be energetic. Apply the interrupted current in preference to the continued one ; but after *twenty* or *thirty* shocks, at the most, allow the patient to have a few moments' repose. Both practice and theory seem to prove, that the interrupted current is more useful than the continued one.

" A pile, with Masson's wheel, or, still better, the electro-magnetic machine, is the most convenient apparatus for this purpose. Electro-magnetic apparatus are now constructed, in which the interruptions of the current are made without the necessity of an assistant.

" We may employ, as conductors, two ribands of sheet lead or copper. The extremities, which are placed in contact with the skin, should be covered with cloth moistened with salt and water. In some cases, it is useful to employ, as extremities of the conductors, acupuncture needles.

"The number of authentic cases of paralysis cured by the electrical treatment, is already sufficiently great to encourage physicians and patients to persevere in its use. Perseverance indeed is indispensable in the application of the electric current, for without it, successful results are impossible.

"*In Tetanus.*—The use of the electric current has been suggested in another malady, namely, tetanus, I believe I am the first who has attempted its application to man.

"The principles on which is founded its employment for the cure of this disease are the following. A current which circulates by jerks in an animal during a certain time, produces tetanic convulsions; the direct current, if continued sufficiently long, produces, on the contrary, paralysis. From this it was concluded, that the continued passage of the latter, in a tetanised limb, would destroy this condition, by producing a state more or less allied to paralysis. The truth of this conclusion is demonstrated by facts. In operating upon frogs which have been tetanised by narcotics or hydrocyanic acid, we observe a fit of tetanus cease under the influence of the prolonged passage of a direct current. The frogs die without presenting those convulsions, which are observed take place when these animals have not been submitted to the direct current.

"The effects produced by the application of the electric current in a case of tetanus, which I published in the *Bibliothèque Universelle* (May 1838), appeared to me in some degree to prove the truth of the scientific principles which I have explained to you. During the passage of the current, the patient experienced no violent convulsions; he was able to open and shut his mouth; and circulation and perspiration appeared to be re-established. Unfortunately, this amendment did not continue long; the disease being occasioned, and kept up by the introduction of foreign bodies into the muscles of the leg. Perhaps more satisfactory results from the electric current may be expected, in cases where tetanus has not been caused by a traumatic injury. Moreover, we ought already to be thankful in being able to lessen the sufferings to which this dreadful disease gives rise.

"*In Urinary Calculi.*—Finally, it has lately been proposed to dissolve vesical calculi, and resolve cataract by the electric current. It is sufficient, however to remember, that the substances which compose urinary calculi are insoluble in water, to be convinced that such an application has no basis to rest on.

"*In Cataract.*—As for cataract, I would remark, that by changing the position of the poles of a current, which has been made to pass through an albuminous liquor, we never find that the albumen coagulated at the negative pole is redissolved at the positive pole. It is possible, therefore, to create a cataract, but impossible to destroy it

"*In Aneurism.*—Petrequin, of Lyons, has recently proposed the use of galvano-acpuncture for curing certain aneurisms. This application appears to be founded on the property which the electric current possesses, of coagulating the serum of the blood, and consequently, of partially filling up the aneurismal sac."—Pp. 273–277.

"*Peculiarities of Electric Irritation of the Nerves.*—I consider it necessary to give here a summary of the principal differences which have been ascertained experimentally, between the effects produced by electric irritation of the nerves, and those determined by other stimulating agents, such as heat, chemical and mechanical actions, &c. The following are these different distinctions:—

"1. Electricity is the only irritant which can excite, at one time sensation, and at another contraction, according to the direction in which it traverses a nerve.

"2. The electric current alone, in passing transversely across a nerve, produces no phenomena due to the excitability of the nerve.

"3. The electric current has no effect on the nerves, that is, it neither causes contraction nor sensation, when its action on the nerve is prolonged.

"4. The electric current alone can modify the excitability of a nerve, and even rapidly destroy it, when the current circulates in a certain direction, and can preserve or augment the excitability, when passing in the opposite direction.

"5. Lastly, of all irritating agents, the electric current is the only one which possesses, for a long space of time, the power of reviving the excitability of the nerve, when it has become very much enfeebled in respect to other stimulants.

"*Analogy between the Electric and Nervous Forces.*—These differences between the action which the electric current exercises on the nerves, and that of other irritants, evidently show that the first is more simple than the others. Hence arises the analogy between the nervous force and the electric current, which the earliest observers of galvanism faintly perceived.

"But ought we, from this analogy, to conclude, that the nervous force is merely an electric current? Let us be cautious in assuming such an inference, which is too often adopted as one of the best demonstrated experimental truths.

"Let us, in the first place, inquire whether, with the instruments which physical science furnishes us, the electrical current can be discovered in the nerves of a living animal? Can this current exist there, and if it does, would it be under the requisite conditions to be endowed with the characters of the nervous force?

"The muscular electric current, on which we have dwelt at some length in one of the preceding lectures, is shown, by experiment, to owe its origin to chemical actions going on in the muscles. We have seen that this current exists in the integrant parts of the muscles, as well as between the molecules of two bodies which enter into combination; that it circulates there without any regularity, and one might say that, as Ampere imagined, it took place in magnetisable bodies, and that it is only by an experimental arrangement that we can discover the presence of this current. We have also shown, that the nerves have no direct influence over the production of this current, and that their office is limited, in experiments on the muscular current, to that of a slightly conducting body, communicating with certain parts of the muscle.

"*No Electric Current in the Nerves.*—It was important to search for the presence of an electric current in the nerves of a living animal. I shall refrain from noticing here all the experiments which have been undertaken with this object in view, and which have terminated in the announcement, at one time, that this current did exist,—at another time that it did not exist. The most conscientious and best established conclusion is this :—*In the present state of science, and with the means of experimenting which we at present possess, no sign of the electric current is found in the nerves of living animals.*

"Some persons have asserted, that steel needles introduced into the muscles perpendicularly to the direction of their fibres, become magnetic, especially at the moment when the muscles contract. From this it has been concluded, that there existed an electric current in the nerves, and that the circuit was established as in a spiral or an electro-dynamic cylinder.

"I have repeated these experiments, by introducing steel or iron needles into the muscles of living animals, and in all directions relative to their fibres. In order to convince myself of the magnetisation of the needles, thus plunged into the muscles, I made use of those of a very good astatic system, and even of those of the sideroscope of Lebaillif. I never obtained an affirmative result. I placed the recently prepared thigh and leg of a frog, in the interior of a spiral of varnished copper wire, the extremities of which were connected with those of a second smaller one, in which there was a soft iron wire. I afterwards irritated the nerve of the frog, observing, at the same time, if an induced current transversed the spiral, and magnetised the iron wire. All my researches were fruitless.

"I likewise tried the effect of introducing into an exposed nerve of a living animal, the conductors of a very delicate galvanometer by two points, as far apart as possible. I operated upon animals under the influence of certain narcotic poisons, and I excited strong muscular contractions in them, at the moment when I placed the two wires of the galvanometer in the nerve; but I must confess that, whenever the experiment was well made, I never obtained evident and constant traces of the electric current."—Pp. 281–284.

"Lastly, I may add that, from what we know of the properties of electricity, and of the laws of its propagation, it is impossible to conceive the existence of a current circulating in the nerves. In order that an electrical current should pass from one extremity of the nervous system to the other, it would be necessary to compare the nerve to a metallic wire varnished or otherwise insulated, an assumption which is not in accordance with fact. An electric current which,

subjected to the will, would set out from the brain to reach the muscles, by traversing the nerves, could not be stopped in its course by the ligature of the nerve; whereas, we well know, that the propagation of the nervous force is prevented by that proceeding. Lastly, its circulation in the nerves requires that the nervous system should form a closed circuit; but the labours of anatomists are very far from having proved such an arrangement, especially in the ultimate ramifications in the muscles, where it would be especially necessary."—P. 285.

"Let us conclude, then, that the electric current does not naturally exist in the nerves of a living animal. The laws of its propagation require conditions which are not found fulfilled in the nervous system; the propagation of its force is interrupted by causes which could not produce a similar effect upon the electric current."—P. 286.

On the Relations between the Nervous Force and Electricity M. Matteucci gives the following statements.

"This unknown force of the nervous system is, therefore, not electricity, and still less is it the electric current. But what connection exists between it and electricity, or the electrical current?

"In order to reply to these questions, I will here sum up, in a few words, the only positive result that my lengthened investigations of electro-physiological phenomena of animals have permitted me to deduce.

"There exists, between electricity and the nervous force, an analogy which, if it does not possess the same degree of evidence, is, however, of the same kind as those analogies which we know to exist between caloric, light, and electricity. We have seen, when speaking of the phenomena presented by electrical fishes, that the faculty which they possess of producing electricity is obedient to the nervous system. There is, then, in these animals a peculiar organic structure, such an arrangement of parts, that, by an act of the nervous force, they can develop the electrical fluid. You remember the identity of causes and circumstances which excite and modify muscular contractions; and this function is proper to these animals. You have seen, that in them the property which they have of giving the discharge is under the immediate dependence of the functions of the nervous system, as well, also, as is the faculty which the muscles have of contracting.

"A crystal of tourmaline, when heated, develops electricity, and from this fact we assume, that between caloric and electricity there exists a more or less intimate relation. The phenomena which we have observed in electrical fishes, prove that a link of the same nature unites the nervous force and electricity. Electricity is not the nervous force, nor is caloric electricity. The latter is derived from caloric in consequence of the form of the integral molecules of the tourmaline; the nervous force is transformed into electricity under the influence of the peculiar structure of the organs of the electric fishes.

"*How does Electricity excite Nervous Phenomena.*—Let us examine, lastly, how electricity can excite nervous phenomena. The excitability of the nerves can also be awakened, and sensations and muscular movements determined, by other agents as well as by electricity; namely, by heat, mechanical and chemical actions. Ought we to conclude from these facts, that mechanical, chemical, and calorific actions, affect the nerves after they have been transformed into the electric current? We have no evidence in favour of such an hypothesis; if, however, notwithstanding this, we would assume an analogous change, there would perhaps be some appearance of probability of it in the case of chemical actions; but none for mechanical and calorific actions. There are no circumstances, in fact, under which we obtain a current by merely dividing a body. It is impossible to establish a comparison between a muscle and a thermo-electric body. In all these actions, we can only see various causes of molecular movement.

"We may, however, ask ourselves this question,—Does the cause of nervous phenomena reside in these molecular movements of the substance of the nerves, or is it owing to a disturbance in the equilibrium of the ether, distributed in the nerves? Is this disturbance the consequence of a particular movement of the ether, which should constitute what we call the nervous fluid?

"We can make no satisfactory reply to these important questions; the facts which are necessary to enable us to resolve them are wanting, and will remain so, perhaps, for a long time to come. Yet, if it be sometimes allowable, in scientific matters, to express, not only convictions, but even doubts, I will not hesitate to tell you that I do not consider it impossible to interpret nervous phenomena, by the mere movement of the ponderable molecules of nerves.

"But rather than stop to develop hypotheses, I believe it will be more useful, before leaving the subject of the nervous force, to dwell for some time upon two classes of phenomena, or of the researches connected therewith. One relates to the fact which I discovered, and named *contraction by induction*, or *induced contraction*; the other relates to the development of the nervous force.

"*Induced Contractions.*—By the term contraction by induction, or induced contraction, has been expressed, in England, a physiological fact which I discovered some years ago. I shall henceforth employ this name, which has the advantage of expressing briefly the phenomenon, and of specifying in some respect its nature.

"I shall commence by briefly stating in what this phenomenon consists, and by detailing the principal researches which I at first made to determine its laws. Having prepared a galvanoscopic frog, we place its nerve upon one or both thighs of a frog placed in the ordinary manner; then, by applying the two poles of a pile to the lumbar plexus of this frog, we observe that when the muscles of the thighs contract, convulsions simultaneously occur in the galvanoscopic claw, whose nerve rests upon the thighs in contraction.

"I have also ascertained that this phenomenon likewise occurs when we place the nerve of the galvanoscopic frog upon the muscles of the thigh of a rabbit, and make these contract by means of a current acting upon the nerve which ramifies in the thigh. I have also observed contractions of the galvanoscopic frog, without the employment of the electric current to excite the contractions of the muscle, producing the induced contraction; the action of some other stimulant, applied to the spinal marrow, or the lumbar plexuses, being substituted.

"Lastly, I have repeated these experiments by placing very fine layers of different substances between the nerve of the galvanoscopic frog and the muscular surface where the induced contraction is developed. A leaf of gold, or a very thin and insulating lamina of mica, or glazed paper, being interposed, prevents the production of the phenomenon; that is to say, the contractions by induction do not occur in the galvanoscopic frog; but, on the contrary, a leaf of fine paper, moistened with water, does not prevent the occurrence of this contraction.

"*Conclusions.*—From the whole of these facts we are authorised to conclude—

"*1st*, That we cannot consider the induced contractions of the galvanoscopic frog to be due to the electric current.

"*2d*, And that we might, on the contrary, assume that an electric discharge happens during the contraction of the muscle."—Pp. 286–291.

M. Matteucci thinks that there can be no longer any doubt that, with an equal number of elements, taken from the same frogs, the muscular current is more energetic than the proper current. He afterwards infers, from the results of eight experiments on frogs, mounted in the usual way, that there is no evolution of electricity during muscular contraction.

"During these contractions, if the experiment be made without any interruption or change in the circuit, the needle of the galvanometer undergoes no variation."—P. 298.

"There now remains to be explained the phenomena presented by the proper current, when we employ entire frogs, and which consist of the almost constant occurrence of signs of augmentation, when we, for the first time, touch the lumbar plexuses of the frogs with potash; whereas, on the contrary, when we apply an acid solution to them, the needle immediately descends. I have repeated and varied for this purpose my first experiments, and the following is the way in which these differences may be explained.

"Whatever be the form of the muscular elements which we use to make the pile, that is, whether they be made with entire frogs, with half-thighs, or such as we have described, when we moisten the surface of the muscular elements with an acid, or alkaline solution, it invariably happens, whether there be or be not contractions, that the deviation diminishes, and that the needle returns to 0°, where it stops, if either the application of the alkali be repeated, or the solution employed be too concentrated.

"This effect is analogous to that already described, and which the muscular elements present when they have been plunged for a few seconds into acid, or alkaline solutions.

"In our mode of experimenting, we excite the contractions in the muscles by touching with alkali those points which are out of the circuit, and which do not constitute parts of the electromotive element.

"In piles formed of entire frogs, with which we most frequently succeed in obtaining, in a transient manner, signs of augmentation in the current, by touching the lumbar plexus alone with alkali, we touch with alkali the parts which really constitute the electromotive element; and even in these cases we never succeed in obtaining the signs of the current, if we moisten the whole of the muscular surface.

"I may also add, that if we employ an acid solution, taking care to touch with a pencil the lumbar plexus only, and not the muscles of the thighs or of the legs, the deviation is not weakened; and notwithstanding the contractions excited, are less strong than those produced by the alkali, there is no augmentation of deviation. In order to cause the needle to descend, it is necessary to touch the surface of the muscles with an acid. This same effect takes place with the alkali, and it is, I repeat, in accordance with the experiments, already related, upon the muscular elements which have been immersed in acid or alkaline solutions.

"It is, then, only with the pile of entire frogs, or by touching with alkali the lumbar plexuses alone, that we often obtain a slight augmentation of deviation; and this phenomenon appears when operating in the same manner with acids. By taking into consideration all the experiments which I have mentioned, this result cannot be regarded as contrary to the negative reply that we have given, in an absolute manner, to the question of the development of electricity during muscular contraction.

"However little may have been the attention paid to the exposition of all these facts, it is impossible not to perceive how great is the difficulty which we encounter when we attempt to explain, in the particular case alluded to, why the alkali produces an augmentation of deviation in the proper current of the pile formed of entire frogs. I am inclined to think that, as the alkali excites in the muscles stronger and more permanent contractions than those produced by acids, the contraction ought in most cases to render the contacts between the elements more perfect, and, consequently, the interior conducting power of the pile ought to be augmented. Indeed, in the pile formed of entire frogs, the contact between the elements is always imperfectly established, and we constantly observe great differences in the intensity of the current produced by these same elements, according to the greater or less care with which they are arranged.

"Whatever may be the explanation given of the slight augmentation manifested in the intensity of the proper current, by touching the lumbar plexus of frogs with alkali, and thereby exciting muscular contraction, it is certain that this fact alone cannot prove that electricity is developed during the muscular current; the more so, as those before mentioned lead us to the conclusion that this development does not occur."—Pp. 299–302.

He then shows by experiment that the induced contractions are excited only by a muscle in contraction; and after detailing various experiments, gives the following summary of the hypothesis which he conceives may be established :—

" *Hypotheses of Induced Contractions.*—After having thus enumerated a long series of experiments relative to the circumstances which intervene in producing, modifying, or destroying the phenomenon of induced contraction, one would hope that, with such a collection of facts, it would be easy to give the physical theory of the phenomenon. Unfortunately, I believe that such is not possible, and this doubt obliges me to discuss minutely the different hypotheses which can be formed to explain induced contractions.

" *1st*, It is sufficient to have once seen this phenomenon produced by provoking the inducing contractions with any mechanical stimulant, to be convinced that the electric current, employed to excite the contraction, is not propagated to the nerve of the frog, and takes no part in the phenomenon. How can the induced contraction of the second and third order be understood? In what way can we explain the fact, that the induced contraction is wanting, even when the current has been, as usual, applied upon the lumbar plexuses, and that only because by the incision of the nerves in the thigh, we have abolished, or greatly diminished, the inducing contraction? Why does the induced contraction fail when we apply the same current in the nerve below the thigh, in which case the muscles of the thigh do not contract? Why, when we operate with the current upon the lumbar plexuses of a frog, already weakened to such a degree that it only excites contractions at the commencement of the passage of the direct current, or at the moment of the interruption of the inverse one; why, I say, is there, in this case alone, induced contraction? It is useless to continue to reconcile the objections we can make to the interpretations of this phenomenon, by having recourse to a diffusion of current to produce the inducing contractions, a diffusion which we can in no way comprehend physically.

" *2dly*, We might suppose that the induced contraction is the effect of a mechanical stimulant, namely, of the contraction of one of the inducing muscles, which thus gives a shock to the galvanoscopic frog.

" I have tried a great number of times, by using very sensitive galvanoscopic frogs, to excite, by all possible means, some movements in the muscles of the thighs, without causing the galvanoscopic frog to contract. If the true cause of this phenomenon really resides in this shock, how can we explain the cessation of the induced contraction, occasioned by the interposition of a very thin leaf of gold, or mica, between the nerve and the muscle? I have very frequently tried the effect of applying the nerve of the galvanoscopic frog upon plates of metal or glass, upon tense membranes, and upon vibrating catgut strings, and have never seen contractions manifested in the galvanoscopic frog. It is not then the shock of the contracting muscle against the nerve of the galvanoscopic frog which is the cause of induced contraction.

"*3dly*, In some extremely rare cases, contraction in the galvanoscopic frog is produced when we extend its nerve upon the thigh of the second frog, where both are perfectly insulated. But whenever this anomaly presents itself, we never fail to discover the cause. It sometimes depends on some portion of the internal part of the muscle being exposed; sometimes on some portion of the muscle being left attached to the nerve of the galvanoscopic frog, and touching the nerve when we place it on the thigh. It appears to me, also, that these contractions are sometimes manifested when we touch the tendinous extremities and the surface of the muscles of the thigh with two points of the nerve of the galvanoscopic frog. I begin by telling you, that induced contraction is invariably obtained in all cases where, from all the precautions that have been taken, there exists no circumstances capable of exciting contraction in the galvanoscopic frog. We also know, that, by dividing with scissors the muscular surface of the thighs, and rendering it by this means perfectly smooth, the contraction by induction takes place, when we apply the nerve of the galvanoscopic frog upon the fresh muscular surface. It is also produced through the skin of the frog, and even by placing layers of insulating liquids between the muscle and the nerve. And we have seen that the insulation produced by them was capable of intercepting the propagation of the proper or muscular currents. How can we suppose now, from these facts, that the induced contraction takes its origin from the circumstances which we have before enumerated, even by admitting that they are rendered stronger, or are excited, by the muscular contraction?

"These circumstances can only arise from a phenomenon of the muscular or the proper current which ought to traverse the nerve of the galvanoscopic frog, even when the latter is surrounded by a layer of an insulating substance, which we have seen cannot be the case.

"*4thly*, The first idea which presented itself, to explain induced contraction, was to admit that there was a development of electricity accompanying muscular contraction. There is a disengagement of heat during the contraction; there may be even cases where light might accompany it, according to the observations of Quatrefages, observations which deserve, on account of their great importance, to be repeated, in order to study more fully all their peculiarities.

"Hence, therefore, a certain degree of analogy would authorise us to regard as probable the production of electricity during the muscular contraction. Moreover, the few experiments which I made when I discovered induced contraction, would be equally explained, and in as satisfactory a manner, by this hypothesis. An insulating body, such as a leaf of mica or glazed paper, prevents, by its interposition, the induced contraction from taking place. The like result occurs when a leaf of gold, discharging perfectly the electricity which we presume is developed during contraction, prevents the nerve from being traversed by it.

"Notwithstanding these first suppositions, which we made in the hope of being able to furnish a simple explanation of induced contraction, and, at the same time, a demonstration of the existence of a very important fact respecting muscular contraction, we are now compelled to abandon entirely this view, since it is disproved by experiment."—Pp. 313–317.

"We are, then, still ignorant of the cause of muscular contraction, and all that we know of this phenomenon are the following particulars:—it is produced, even when acting at great distances from the muscle, upon the nerve, whose ramifications it receives; the integrity of the nervous filament, from the point where the excitation takes place to the muscle itself, is indispensable; this transmission is effected with such rapidity, that we are compelled to compare it to that of electricity, light, and radiant caloric, propagating itself through various media; what modifies, augments, or destroys the accomplishment of the chemical physico-phenomena of the nutrition of the muscle, has an analogous action upon its contractibility, provoked by any influence acting upon the nerves; and lastly, in the laws of the contraction of a muscle, we find an analogy with the physical laws of elasticity.

"The fact of induced contraction will, then, be a phenomenon of induction of this unknown force, which circulates in the nerves and produces muscular contraction.

"By assuming as well-proved, that the phenomenon of induced contractions could not be satisfactorily explained by having recourse to the electric current, as I believe I have completely placed beyond doubt, it appears to me that we cannot, in speaking of a fact as simple as that of induced contraction, assume any other interpretation than that which we have given. The induced contraction will be a new phenomenon of the nervous force, a phenomenon of which we have already given the principal laws. To me it would seem most rational henceforth to call that *muscular induction*, which we have hitherto termed induced contraction.

"A muscle in contraction exercises an inductive action upon a living nerve. After all, I am compelled to say that, recently, I have resumed the examination of the induced current, by considering it as due to a very feeble electric discharge analogous to that of the Leyden jar. After having seen that excessively feeble discharges produce contractions in frogs, knowing that the presence of these discharges cannot be detected either by the galvanometer or any other instrument, but only by the frog, it appeared worth while to ascertain whether a very slight discharge of the Leyden phial, traversing a muscle, acted on the nerve of the galvanoscopic frog, and under the same laws that we have found induced contraction to do. I must admit that, notwithstanding a great number of endeavours, I have not been able to discover any differences. I wish, then, to tell you frankly, that until new facts are obtained with regard to induced contraction, we cannot decide whether it be due to a nervous induction, or be the effect of an electric discharge occurring during contraction.

"If we could succeed by experiment in proving the truth of the latter hypothesis, we should have made a grand step in the analogies between muscular contraction and the electric function of fishes."—Pp. 319–321.

The following extract contains some interesting inferences:—

"*Production of Nervous Force.*—In conclusion, let us say a few words on the production of the nervous force. Although it be true that we possess no knowledge of it except in living animals, and, consequently, want the apparatus to accumulate it and study its laws, out of the animal itself, yet we should not abandon all physical analogies in the investigations which we make relative to its mode of production.

"Whenever a movement occurs, or the effect of force is manifested, we are certain that some transformation of matter must have taken place. Wherever a force is exerted, a chemical action precedes it. Caloric, electricity, and light, furnish us, at every instant, with evidence of this truth. But putting aside all analogies, let us examine the conditions under which the development, of what we call nervous force, takes place in animals. A man or animal, after a long walk, having put his machine into motion, is fatigued, and needs repose and nourishment. Although it be true that facts are wanting to establish an intimate and really scientific connection between the effects of labour, repose, and nourishment, on the one hand, and of the loss and reparation of the nervous force on the other, yet we cannot abstain from discussing these facts, complicated though they be, on the principles of mechanics and general physics.

"Muscular exercise, of whatever kind, is constantly followed by a loss of power, and as we see the animal machine recover its aptitude for exercise, after having obtained food and rest, we must admit that the force necessary to muscular action may arise from the chemical actions of nutrition; inasmuch as, by means of the latter, and of repose, this force is reproduced and accumulates in the nervous system. Interrupt for a certain time the sanguineous circulation in a muscle, and soon this becomes incapable of contracting; but with the return of blood the muscular force revives. In animals, where circulation and respiration are very active, the development of muscular force is more considerable.

"But among the numerous chemical actions that occur in animals, which is the one that gives rise to the force which is put in action during muscular contraction? It is impossible to give a satisfactory answer to this question.

"Physiologists now admit that heat is produced by the combustion of fatty matters, and principally by that of bodies into which fecula is transformed during digestion: the nervous force may be due to chemical actions which take place during the changes which the neutral azotised substances of the tissues undergo. But I know of no experiment which directly proves this difference of origin between heat and the nervous force.

"Of all chemical actions of which the animal is the seat, the only one which we perfectly know, and which we have even measured, is that which produces carbonic acid. On the average, man converts and exhales, in the form of carbonic acid, 10 to 15 grammes of carbon per hour.

"Setting out with these data, we shall endeavour to compare the nervous force which results from this chemical action, by representing the mechanical work done by a man in the space of one day, with the quantity of work that this same action could produce in the same space of time, either by means of heat, or by electricity developed by it. In other words, it is possible to ascertain whether we obtain with steam engines, or electro-magnetic apparatus, and by means of a determined chemical action, a mechanical effect equal to, or different from, that which is produced when this same action takes place within an animal.

"But before commencing this investigation, I cannot refrain from observing, that, in order to establish this comparison, it is necessary to assume one of the two following hypotheses: we now know that heat, electricity, and nervous force, are developed in animals, and we assume that the causes of their production reside in the chemical actions of nutrition. But we may suppose that they are produced in certain constant quantities, and independently one of the other; or, indeed, that from a certain chemical action there never follows but a certain quantity of force, whatever be the form in which it is manifested.

"In order to make myself better understood, I will give an example. Zinc burns in oxygen, producing light and heat; this same zinc can be oxydised by decomposing water and developing only heat, or heat and electricity, if we touch it with a platinum wire. Suppose, now, that we could transform these forces into a certain quantity of mechanical work done by them, we might say that the sum of these quantities is the same in every case, and that when one happens to fail, the other is substituted for it, by a relative quantity drawn from their mechanical equivalents. But it might equally happen, that they were developed independently of each other. Experiment would reply in favour of this latter opinion. I measured the heat disengaged by zinc oxydised by decomposing water, and I repeated this experiment by having, in addition, the disengagement of the electric current; the heat was invariably the same, and nearly equal to that which zinc produces when oxydising by burning in oxygen.

"We might then consider all the chemical action of carbon which combines with oxygen in animals, as the cause of nervous force, independently of the heat and electricity that it may produce; and we would ask whether this action, taking place in the animal itself, determines an effect analogous to, or different from, that which it would determine, if it occurred in a steam-engine or in an electro-magnetic apparatus.

"While travelling on one occasion with the celebrated Robert Stephenson, we were obliged to send a man on foot forty miles. I asked Mr Stephenson what quantity of carbon was necessary to transport a man forty miles by a locomotive. He replied about 5 killogrammes [about 11 lbs. avoirdupois].

"The person we had despatched accomplished his journey, by walking, in less than ten hours, consuming by his respiration a quantity of carbon not exceeding 150 grammes, that is about $\frac{1}{34}$ of the quantity which would have been necessary if this transit had been effected by a locomotive. M. Dumas has calculated how much carbon would be burnt in a steam-engine, in conveying a man from the level of the sea to the summit of Mount Blanc. The quantity would be from 1000 to 1200 grammes; but a man accomplishes this feat by a two days' march, and consumes only 300 grammes. The difference in the second example is not so great as in the first; because the useful result which we obtain from a stationary steam-engine is much more considerable than that from a locomotive. It is equally true that the difference is very great, and that the work produced from nervous force, derived from a certain chemical action, is much greater than that which this same action produces when converted into heat.

"I can show you in another way the great advantage which results from the transformation of chemical action into nervous force in an animal.

"I endeavoured to measure the amount of mechanical work obtained, when applying to the nerves of a frog a current produced during the oxydisation of a given quantity of zinc in a pile. Here are the numbers obtained: three milligrammes of zinc, oxydising in one day, furnish a current which, if we suppose that it could be continually applied to the nerves of a frog, would produce a muscular power equal to 5·419 killogrammes, raised to one metre in height during the same interval of time. It is probable that these numbers are far from being accurate, and I intend hereafter to repeat these experiments: it is, however, certain, that the causes of error are all one way, and tend to represent as much smaller than it really is, the effect produced by the three milligrammes of zinc.

"The same quantity of zinc burnt, would yield a quantity of heat, which employed in forming steam, would execute work equivalent only to 0^k, 8304, raised to one metre.

"Finally, the current produced by the three milligrammes of zinc was applied to an electro-magnetic machine; and in this case we obtained 0^k, 96, raised to one metre.

"Everything, then, leads us to the conclusion that the mechanical work developed by chemical action, and transformed into nervous force, in an animal, is very great; and that, in all the machines which man has invented, he is always, and will be perhaps for a long time to come, far from attaining that degree of perfection which exists in those machines which we know not how to imitate and can only admire."—Pp. 321–326.

The facts regarding the circulation of the blood, and the different shares which its several agents possess in carrying on that important function, are generalized in the following manner:—

"I consider it useless to detain you longer for the purpose of experimentally demonstrating, that the contraction of the heart and the elasticity of the coats of the vessels, especially of the arteries, are the principal powers of the circulation. If we tie an artery in a living animal, the vessel almost entirely empties itself of blood, and the circulation continues in it only for a short space of time. The contrary happens with the ligature of a vein; for in that case the blood soon accumulates, and the vein swells below the ligature. I shall confine myself to the relation of a single experiment by Magendie. The crural artery and vein of a dog were laid bare, and a ligature applied to the vein; an incision was then made below the ligature, and a jet of blood escaped. When the artery was pressed between the fingers, in order to prevent the passage of the blood within it, the jet of venous blood diminished, and was ultimately stopped completely; but when the pressure was removed, the jet re-appeared. These phenomena could be reproduced several times. The conclusion drawn from this fact is evident: the blood traverses the capillaries, and circulates in the veins by the sole forces which have propelled it into the arteries, namely, by the contraction of the left ventricle, and that of the arterial coats, which are the chief powers, the only ones, in fact, of the circulation.

"The influence which atmospheric pressure exercises upon this function is very limited. I have already mentioned that at each experiment the column of mercury of the hæmo-dynamometer, applied to the artery, rises; and at each inspiration it falls. Poiseuille observed the same phenomenon, and under the same circumstances, in large venous trunks; thus, in the jugulars the column of mercury rises during expiration, and falls during inspiration. These phenomena do not occur when we make the experiment upon venous trunks distant from the thoracic cavity. We easily understand that when the latter dilates, the pressure of the atmosphere must compress the veins, and thus, by means of valves, so placed in these vessels as to impede the return of this liquid, it assists in making the blood move towards the heart. On the contrary, during expiration, when the thoracic cavity is contracted, all the vessels contained within it are simultaneously compressed. Experiment has shown that the variations of the pressure of the blood in the arteries and the veins correspond to the respiratory movements, and cease to manifest themselves in the sanguineous trunks situated beyond this cavity.

"The muscular contraction of the heart, and the contraction of the arterial coats, are then the principal motive powers of the sanguineous circulation. The combination of these two forces in the mechanism of this function is so perfect, that the movement of an intermitting jet, produced by the alternate contractions of the heart, is transformed into a continued movement by the elastic force with which the arterial coats are endowed. By this force, the arteries, which were at first dilated, recover themselves, and consequently propel the blood forward at the moment when the contraction, which has thrown the blood into the arteries and caused them to dilate, ceases.

"Let us imagine, then, a circular system of tubes of different diameters, and having elastic sides; and that the two extremities or apertures of this system open into two cavities separated from each other, and of which the walls can approximate and separate like those of a pair of bellows. When we fill this tube with a liquid, and rapidly close one of these cavities by depressing its moveable wall, the liquid which it contains is urged into the tube, and thus pushes onward the liquid column. This advances by a movement which soon becomes uniform, and is rapidly communicated to the entire mass. In the meantime, the other part of the bellows opens; the liquid which filled the tube in the opposite extreme advances also, and is easily projected into the dilated cavity. If the walls of the tube were not elastic, the movements would be intermittent, and would cease as soon as the bellows could no longer open; but they become continuous, because the walls are endowed with that property which begins to be exercised precisely at the moment when the bellows shuts, and the action lasts during the whole time that they remain at rest. The function which I have described, by supposing it to take place with a pair of bellows, is that which is performed by the heart. The walls of the left ventricle thus approach each other, contract with a great rapidity, which can be determined when we know the exact duration of the contraction and the quantity of blood expelled. The capacity of the ventricle is thus diminished, and a certain quantity of blood thrown into the aorta, and a movement communicated to it which is propagated throughout the system. At the instant that the arteries dilate, the right ventricle opens, and the blood enters it. The contraction of the left ventricle ceasing, the arteries return to their original condition, and again propel the blood forward.

"*Pulmonary Circulation*—We have passed over in silence all that which relates to the pulmonary circulation, this being produced by the same causes, and under the same laws as the circulation in the rest of the body.

"*Conclusion.*—Thus, then, by the arrangement of the various parts of the sanguineous system, is solved, by a very simple mechanism, and conformably to the physical laws of the movements of fluids, a very complicated hydraulic problem; namely, that of the continued distribution of the same liquid in a system of tubes, having different diameters in different parts of the body, and, consequently, with very variable velocities, and always in relation to the functions of these parts, and all this by the simple alternating impulsion given to this fluid by the sudden contraction of a species of sac, which makes a part of the tube itself."—Pp. 365–367.

It has appeared to us more likely to present a just view of the doctrines and inferences established by M. Matteucci, if we place them before our readers, as nearly as possible, in the language of the author, than by attempting to furnish any abstract of our own; and we trust that readers who peruse the foregoing extracts, will be not only enabled to form a correct opinion of the doctrines peculiar to the author, but will also be induced to study carefully the treatise itself. It would be presumptuous to attempt to offer either commentary or critical remark. No one, it may be remarked, is entitled to do so, who has not bestowed on the subject the same attention as the author, and who does not possess a knowledge equally accurate and extensive of physical and physiological science. We may, nevertheless, be allowed to observe, that the author has examined the entire subject in a spirit of the most cautious deliberation and the most sound judgment; and we are confident, that if his distinctions and arguments are thoroughly weighed, physiological speculators will understand more accurately than heretofore, the exact limits between physical and physiological phenomena in living organic bodies. The observations on the differences between crystallisation and organisation, which are given at page 191, may be advantageously considered with those which were adduced in last volume, when speaking of the hypothesis of the late Mr Coleridge. Particular instances, however, of just and rational distinctions, properly enunciated, it is superfluous to adduce. The different parts of the work should be read in connection and in comparison with each other; and readers will perceive, that Professor Matteucci is at the same time a well-informed philosopher and a sound reasoner.

This book, however, we do not recommend for the novelty of its contents. The arguments and views are not so much new, as methodically, distinctly, and logically stated. Some facts are new; for instance, that regarding the light of the glow-worm, which is inserted in the Medical Intelligence. This, however, is not common. Its general character is, that everywhere facts, well known and more or less established, are placed in such a manner as to furnish new and more correct inferences; and other facts, which had been believed to be established, are modified, or rectified, so as to furnish more accurate conclusions than it had been previously possible to obtain. The valuable parts of these lectures are the precision with all physical phenomena are examined; the judicious manner in which those phenomena which are purely physical, are distinguished from those which may be called vital and organic; and the perspicuous course, by which the reader is imperceptibly taught to understand that, which he had always partly imagined, but never thoroughly comprehended.

Art. III.—1. *On the Employment of Nitrate of Potash in Acute Rheumatism; with Suggestions for the Use of Saline Solutions as External Applications in Local Rheumatic Inflammation.* By W. R. Basham, M.D., Physician to the Westminster Hospital. (Medico-Chirurgical Transactions, vol. xxxii., xiv. of second series, p. 1, Art. I. London, 1849. Read, May 9. Read, November 14, 1848.)

2. *The Treatment of Rheumatic Diseases by Lemon Juice: with Illustrative Cases from Hospital Practice.* By G. Owen Rees, M.D. F.L.S., Fellow of the Royal College of Physicians, Assistant Physician to, and Lecturer on Materia Medica at, Guy's Hospital, Physician to the Pentonville Prison, &c. London, 1849. 8vo. Pp. 38.

Acute rheumatism is not a very incurable disease. Under the employment of blood-letting, laxatives, nauseating remedies,—as tartrate of antimony,—or eccoprotics and diaphoretics, as calomel and James' powder, followed or associated with opiates, with due regulation of diet,—its urgent symptoms usually subside in the course of two or three weeks. Yet it is a disease fraught with great anxiety to the practitioner, and much present suffering to the patient; and it is impossible in any given case to say, whether the patient is to get perfectly well, without suffering any sequelæ or effects of the disease. Either the disease may be complicated with, or may give rise to, *Pericarditis*, or to *Endocarditis*; or the affection of the valves, in consequence of the latter disorder, however transitory and slight, may lay the foundation of valvular disease; or in that manner, or without valvular disorder, it may give origin to hypertrophy of part or the whole of the organ. The disease may also cause meningitis and arachnitis; or it may induce affections of other internal membranes and organs. Acute rheumatism in this manner becomes a disease fraught with great danger and of doubtful issue, not so much by itself as by reason of its complications, its terminations, and its metastatic transitions. Notwithstanding all these probable and adventitious evils, however, the acute disease is treated with comparative facility.

Much more unmanageable are the sub-acute and the chronic forms of the disorder. Whether it be from the insidious and gradual manner in which these two forms make their approach, or from the parts which they attack, it becomes a much more difficult manner, in many instances, to dislodge them. That form of the disorder which affects the capsules of the joints and the synovial membranes, has long been a perplexing form to the physician. Proofs of the justice of these views may be found in detail in the articles on the work of Dr Roderick Macleod and that of Dr Bentley Todd, of which accounts were given in the fifty-seventh* and sixty-first† volumes of this Journal.

To some, therefore, it may appear less necessary to propose new or unusual remedies in the acute than in the sub-acute and the chronic forms of the distemper. The best answer to this objection is, that, if upon more correct and more thorough knowledge of the nature of the disease, it may be possible to render treatment more precise and more direct, and thereby to shorten the course of the disorder, as well as abate the sufferings of the patient, and prevent the disease from leaving after it serious organic changes,—it is always increasing the powers of medical therapeutics, and rendering more available the resources of the physician.

Dr Basham allows that in the administration of the nitrate of potash in acute rheumatism there is nothing new; and in recommending the use of this salt to the notice of the profession, he is influenced rather by the hope of determining its value as a therapeutic agent than by the desire to claim any originality. The nitrate of potash was indeed largely used by Dr Brocklesby, and by others since his time; and the observations thus collected show not only the success with which it has been administered, but the great extent to which the system, during inflammatory diseases, will endure the action of saline medicines, and particularly that of nitre, with the utmost advantage to the duration and final issue of the disease.

Analysis of the blood in inflammatory disorders has established the fact, that in them a remarkable deviation from the average proportion of the component parts of healthy blood takes place; and that in these diseases this deviation is constant, and uniformly in one direction; namely, that it consists in an excess of fibrine, and an absolute deficiency of the saline ingredients.

Dr Basham has carefully analysed the blood in many instances of acute rheumatism; and he gives the results in three, as showing the average nature of the change. In one man of 30, strong and muscular, one thousand parts of blood gave 6·3 of fibrine and 7·4 of saline matters. In one man, a coal porter, of 45, tall and muscular, one thousand parts gave 8· parts of fibrine and 8·93 of saline matters. In the third, a man of 27, one thousand parts of blood gave 7·2 of fibrine and 9·104 of saline matters. The average of these three instances is 7·163 of fibrine and 8·478 of saline matters, which, we are to understand from Dr Basham, may be taken to represent the average of these ingredients respectively in rheumatic blood.

This fact of the absolute deficiency of saline ingredients in the blood of rheumatic patients Dr Basham adopts as the basis of the explanation of the therapeutic action of nitrate of potash. So long as fibrine is in excess, and the salts are both relatively and absolutely deficient, fibrinous exudation is, he infers, liable to take place, and the most destructive consequences of inflammatory action are to be apprehended. The administration of nitrate of potash, he argues, contributes to restore the balance of the saline ingredients, and to control the tendency to fibrinous exudation; and the progress of inflammatory action being thus retarded, time is gained for other remedies to effect the diminution of the fibrine present in the blood, as well as the destruction of the rheumatic element.

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* Vol. lvii. p. 444.

† Vol. lxi. p. 130.

This is the hypothesis. A small defect of logic, some will say, is perceptible; and admitting the premises, the question is, are the conclusions directly deduced? The blood is deficient in saline ingredients; the saline ingredients of the blood are soda and muriate of soda, or chloride of sodium, and phosphate of iron, chiefly. If these are deficient, then what ought to be supplied is chloride of sodium, and probably carbonate of soda, not nitrate of potash.

It is probable that Dr Basham anticipated this objection; for he observes that there are many reasons why nitrate of potash should be administered in preference to such salts as chloride of sodium, sulphate of soda, phosphate of soda, carbonates of potash and soda, and others which out of the body exercise a similar effect upon the coagulability of the blood.

We know not what has become of Archbishop Whateley. Here certainly he is of little use. If there be many reasons for administering nitre in preference to other salts, then these reasons lie not in the deficiency of the saline ingredients, but in some other circumstances. Let us have the reasons, and let us not be told the blood is deprived of its saline ingredients; yet, instead of giving back these in substance, another salt entirely is proposed to be given.

The truth is, that, in this instance, the facts of Dr Basham are better than his reasons; and we suppose that the best plan is to take the facts, without attaching much importance to the reasons. He reminds the reader, however, that nitrate of potash, even in large doses, exerts but little effect on the secreting organs, except the skin and the kidneys, and often on them it exerts no very obvious action. It does not purge, as the sulphate and phosphate of soda; nor does it induce thirst, as the chloride of sodium; neither does it disturb the digestive functions, a result which, he thinks, is often consequent on frequently repeated doses of the carbonates of potash or soda, especially administered in doses sufficiently large to affect the amount of the saline ingredients of the blood.

Further, it renders the blood fluid, and tends to suspend the separation of fibrine, according to the observation of Dr Stevens.

Facts show that nitre has long been in use among physicians for the treatment of inflammatory disorders. As a refrigerant, as it is called, it was used by Sydenham and others of that time; and it has never been out of use among the physicians of the different German states, in the treatment of pneumonia, bronchitis, and other inflammatory disorders. It was called a refrigerant, because it creates upon the tongue and in the mouth a cooling impression; and many remedies have been endowed with this title of refrigerant for less sufficient reasons. It was known to be capable of preserving animal matter, and therefore it was called antiseptic; though it is by no means proved that what is antiseptic in dead animal matter exerts antiseptic properties in the living body. Some speculatists at one time thought it might be useful in diseases in which oxygen is deficient, as scurvy and purpura, in consequence of the amount of oxygen which it contains, both in its acid and its metalloid base. It was with some notion of this kind that it was latterly employed in the treatment of scurvy by Dr Andrew Henderson, who used to obtain it by elutriation from gunpowder.

Nitre was employed as a refrigerant by Hoffmann and Stahl. The former, who observed that it was not liable to coagulate the animal fluids, bestows a whole section *de præstantissima nitri virtute.*[*] After the example of these authorities, and from daily experience, Dr Lewis assigned it a principal place among medicines of the antiphlogistic class, and observes that it is in general use in disorders accompanied with inflammatory symptoms, whether chronic or acute, and as a corrector of the inflammation or irritation produced by stimulating drugs.[†]

Huxham, one of the most judicious physicians of the seventeenth century, was in the constant habit of giving nitre in agues along with bark (p. 24), in inflammatory diseases in general, in pneumonia, and in pleurisy. Of its use in the former, he says, "nitre, that very necessary cooling attenuant, is very agreeable, administered in either of these compositions;"[‡] and, in speaking of the treatment of the latter, he states that nitre both cools and attenuates the blood.

In 1758 and 1763, Dr Brocklesby, an army physician of much experience, published his Œconomical and Medical Observations; and in that work he gave an account of the use and effects of nitre in considerable doses, in the treatment of the acute rheumatic attacks to which soldiers are exposed. His method was to prescribe for each, as drink, water-gruel, in each quart of which were dissolved two drachms of nitre with or without sugar. He states, that often these men took ten drachms or more of nitre in twenty-four hours. "As I have proposed," he adds, "nitre to be tried hereafter in quantities so much greater than have been usually prescribed, I will, as an apology for my presumption in the matter, appeal to experience. I am assured, from numberless instances, that, in stout young men, by taking 600 grains daily (ten drachms) for four or five days successively, and diluting plentifully, plain nitre proves the most powerful and best sudorific in such complaints; and this quantity, or even more, may be retained in the stomach, and pass through the course of the circulation, by only diluting profusely. My own stomach will, almost at any time, bear half an ounce of nitre, or sometimes six drachms, dissolved in or diluted with three pints of smooth well-boiled water-gruel; and this quantity, swallowed in about half an hour or an hour, proves to me almost an unfailing antidote in the beginning of any incidental fever from severe cold.*

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[*] Hoffmann de Salium Mediorum Virtute, § 13, and § 5.

[†] An Experimental History of the Materia Medica, &c. By William Lewis, M. B., F. R. S. London. 2d Edition. 1768. 4to. P. 403.

[‡] An Essay on Fever, &c. By John Huxham, M. D., F. R. C. Physicians at Edinburgh. 3d Edition. London, 1757. Pp. 204 and 254.

Dr Macbride, who was professor of the Practice of Medicine at Dublin, classes nitre among sedative medicines, and states, that "in common practice nitre is seldom given in large enough quantities to do any remarkable service." In inflammatory fever, and especially in rheumatic fever, he considers nitre to be very efficacious when taken to the amount of six, eight, or ten drachms in twenty-four hours. He mentions the necessity of free dilution, and forgets not to say that the patients who derived advantage from this species of treatment were robust in habit and in military hospitals. It looks not well that no reference is made to Dr Brocklesby; and Dr Basham justly remarks, that little doubt can be entertained on what foundation this opinion was formed. In another passage Dr Macbride adds, that "still later observations show, that the stomach can bear considerable quantities of nitre without detriment;" and under the head of *Febris Inflammatoria Rheumatica* he repeats the statement, that large doses of nitre have been found to procure speedy relief.†

In 1789 and previous to that era, Cullen, who applied to the articles of the Materia Medica a severe form of criticism, not unmixed with disbelief in the powers ascribed to many of these articles, while he allowed that nitre was esteemed one of the most powerful refrigerants, maintained that like other refrigerants it proved directly stimulant to the alimentary canal, and required to be given in minute doses. He, accordingly, rather depreciated the testimony and opinion of Dr Brocklesby as to the endurance of the large doses.

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* Œconomical and Medical Observations. By Dr Brocklesby. In two Parts. London, 1758 and 1764. P. 121.

† A Methodical Introduction to the Theory and Practice of Physic. By David Macbride, M. D. Dublin, 1772. 4to. P. 273 and 335.

"I do not doubt," he says, "but the practice of Dr Brocklesby may be often successful; but I could never find it convenient to imitate it, as I could hardly, or at least seldom, find a stomach that would bear half the quantity of nitre that he seems to have employed; and in most cases, I have been limited in the doses of nitre that I could exhibit. I believe that the employment of nitre, as recently dissolved, will be a more powerful refrigerant than when the solution of it is entirely finished; but I am of opinion that the practice has no advantages to compensate the trouble that otherwise attends it."*

Notwithstanding this decision, the practice recommended by Dr Brocklesby was imitated, if not in the large doses in which he gave the remedy, at least in the administration of nitre, both for inflammatory diseases and also for acute rheumatism. The truth is, that the use of nitre had never been entirely given up, at least abroad.

Thilenius, a Saxon physician, who published, in 1789, at Frankfort, a series of Medical and Surgical Observations, and of which a second edition was, after his death, republished by his son in 1811 and 1812, gave for the cure of acute rheumatism, nitre largely and in doses similar to those given by the English physician, and he speaks of its beneficial effects in favourable terms.

After Thilenius, A. B. Ranoe, a Danish physician, in his account of the diseases observed at Copenhagen in the course of the year 1789, and which he communicated to the Royal Medical Society of that city in 1790 and 1791, states that in cases of acute rheumatism, either after blood-letting or without that evacuation, he trusted much to the use of nitre in large doses, and diluted with much water. He appears, nevertheless, not to have confined himself to this salt. His words are the following:—"Besides, that is, after general and local bleeding by means of leeches, in order to break the force of the fever and to expel the rheumatic matter, the remedies which proved most efficacious, were nitre in large doses diluted with much drink, sal-ammoniac, nitrous clysters, tartar emetic in minute doses, and also *Spiritus Mindereri* and camphor.†

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* A Treatise on the Materia Medica. By William Cullen, M. D., &c., Edinburgh. London, 1789. Vol. ii. p. 343.

† "Praeterea ad vim febris infringendam materiamque rheumaticam expellendam, maxime efficacia sese monstrarunt nitrum in magnis dosibus cum multo potu diluente, sal ammoniacum, clysmata nitrosa, tartarus emeticus fractis dosibus propinatus, necnon spiritus Mindereri et camphora."—Sciagraphia Morborum quos anno 1789. Observavit A. B. Ranoe, M. D., a Societ. Med. Ord. Particula ii. Apud Acta Regiae Societatis Med. Havniensis. Volumen 3ium. Hafniae, 1792, p. 344.

The person, however, who speaks most frequently and decidedly of the use of nitre in inflammatory diseases, and especially in rheumatism, is Joseph Frank, in his Report of the Clinical Institute of the University of Wilna, the chief city of Lithuania, for the years 1805, 1806, 1807, published at Leipzig in 1808. The population of this city, which was at that time above 35,000, nearly two-thirds being Catholics, and one-third Jews, were very subject to catarrhal and rheumatic fevers, to typhus and to peripneumony. Joseph Frank employed nitre very extensively. It is mentioned as having been administered in the treatment of gastric fever (p. 62), apoplexy (p. 73), peripneumony (cap. xi. pp. 81, 82, 83), in intermittent fever (cap. iv. 39, T. II.), and acute rheumatism (cap. xxiv. p. 131). It appears to have been chiefly in the last disease that he gave large doses. Thus in one case he gave, after blood-letting and an emetic, from half a drachm to three drachms in twenty-four hours for about ten or twelve days. Repeated emetics had been the practice in this disease, pursued at Berlin under Horn. The tartar emetic, however, he found, produced not vomiting, but diarrhœa. And as the patient suffered much from squeamishness, he preferred employing nitre, —a remedy, he adds, which, when continued sufficiently long, he had learned, from much experience, to be superior to all others in acute rheumatism.[*]

He subsequently adds, in speaking of two other cases of acute rheumatism, that, at first, he prescribed venesection, but, at the same time, he had recourse to nitre liberally,—" a remedy," he adds, " which, taught by extensive experience, I consider most appropriate to acute rheumatism, above all other inflammatory diseases. For nitre causes a large secretion of urine; and this never takes place without remarkable relief to the patient."[†]

The same physician, in his learned and useful work on the Practice of Medicine, expresses himself with equal decision and confidence on the therapeutic properties of nitre in acute rheumatism. " The internal use of nitrate of potass, from one drachm to half an ounce in the space of twenty-four hours, proves to adults, in whom the stomach is not irritable, an important remedy."[*] And subsequently, he adds: "Nitre, the more diuretic it is, the more beneficial is it wont to be."[†]

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[*] " Quum tandem aeger multum a nausea passus est, potius ad Nitratem Potassæ confugi, quod remedium diu continuatum, omnibus aliis, in rheumatismo acuto palmam praeripere, per diuturnam experientiam sum edoctus."—Acta Instituti Clinici Cæsareæ Universitatis Vilnensis. Auctore Josepho Frank. Annus primus, p. 132.

[†] " Rheumatismi acuti duo exempla in Instituto nostro pertractata, nihil memorata dignum exhibent. Ab initio morbi sanguinem e vena duxi, quam operationem urgente necessitate in aliis bis terve repetere soleo. Simul vero nitratem potassæ larga manu exhibui, quod remedium rheumatismo acuto, præ aliis morbis inflammatoriis, experientia satis ampla, appropriatum judico. Potenter enim nitras potassæ in hoc morbo urinam movet, quod nunquam sine aegri levamine fieri solet."—Annus secundus, p. 49.

Other testimonies of the same kind, showing the great extent to which the salt was occasionally given, might be adduced. The practice, nevertheless, was not in general use; and Dr Richard Pearson, the author of the Practical Synopsis, inferring that the salt was not or could not be given in large doses, assigns, in 1808, for this, the reason that, "The present generation, whose moving fibres are more easily acted upon than those of the last and penultimate race, cannot bear this medicine in the large quantities in which it was formerly prescribed by *Sydenham*, and has latterly been given by *Brocklesby*. More than fifteen grains, taken at once, generally create disturbance in the stomach and bowels, and have a laxative instead of a diaphoretic effect."[‡]

It is a good instance of the uncertainty of many statements regarding the physiological effects, and not a few concerning the therapeutic effects of medicines, that the accuracy of the one now mentioned as delivered forty years ago has been more or less decidedly controverted by subsequent experience.

M. Martin Solon employed nitrate in large doses without its producing any unfavourable symptoms. In rare cases only did it either disturb the stomach or act on the bowels. The salt was given in quantities varying from half an ounce to two ounces in twenty-four hours, dissolved either in lemonade, or in some pectoral ptisan. The effects in thirty-three cases were the following,

On the circulation the effects were not remarkable.

Sweating in a marked degree took place in ten patients. This phenomenon, M. M. Solon thinks, belongs rather to the disease than the remedy.

The urinary secretion was sensibly augmented only in five cases among thirty-three. The nitrate was detected in the urine of every case in which it was sought for.

On the digestive organs no effect was produced in twenty-four cases. In nine it produced nausea, vomiting, or looseness, results which, however, rapidly subsided. In all the patients, after the acute rheumatism had subsided, appetite quickly returned, the digestive functions easily recovered their activity, and the general powers returned to their natural and healthy standard.*

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[*] "Interne nitras potassæ a drachma una ad unciam semis nycthemeri spatio adultis ventriculo non admodum sensili instructis præcipuum remedium sistit." —Praxeos Medicæ Præcepta. Partis I. Vol. III. p. 358.

[†] "Ipse nitras potassæ quo magis urinam movet, eo magis prodesse solet."—Praxeos Medicæ Præcepta. Ibid. p. 359.

[‡] A Practical Synopsis of the Materia Alimentaria and Materia Medica. A New Edition, comprising the Latest Improvements in the London, Edinburgh, and Dublin Pharmacopœias. By Richard Pearson, M.D., Memb. R. C. Physicians, London, &c. London, 1808. 8vo. Pp. 253.

Dr Basham commenced the use of nitrate of potash in acute rheumatism in 1843, before the results of M. Martin Solon's experience had been published; but, encouraged by that physician's report, he continued to employ it in large doses on the principles already stated.

The results of this mode of treatment, whether it be considered in relation to the duration of the paroxysm, or the immunity from the serious concomitants and consequences, may be estimated from the following summary of the effects of the salt.

One, two, even three ounces of the nitrate, freely diluted, may be taken in the twenty-four hours in cases of acute rheumatism; and, in the majority of cases, without producing any obvious effect on the force or frequency of the pulse, the integrity of the digestive functions, the state of the abdominal organs, or even upon the quantity of urine excreted.

The amount of urine passed in the twenty-four hours, although seldom increased in quantity, always acquires a high specific gravity, 1·026—1·040, dependent in great measure on the presence of the nitrate of potash, which can always be detected in that fluid.

The internal use of the nitrate relieves, in a most marked manner, the swelling, heat, and pain of the joints in this disease; and this alleviation has, in certain cases, been observed to take place irrespectively of any other measures; in cases, for instance, in which neither opiates, calomel, antimony, purgatives, nor bloodletting were employed. There seems also to be a certain amount of exemption from cardiac complication, among those who have taken nitre largely in the earliest stage of the inflammatory attack; and exocardial and endocardial inflammation, when it is present, appears to become more manageable, and more easily brought under the influence of other remedies, when the nitre has been simultaneously employed. Dr Basham never saw the nitrate of potash, in large or small doses, produce either nausea or vomiting. In a few cases, pinching or griping pains of the abdomen, with a few watery evacuations which quickly ceased on discontinuing the salt, are the only unpleasant or undesirable effects that he witnessed.

Free diaphoresis is perhaps the most frequent apparent effect; but strong acid perspirations are so common in acute rheumatism, independently of the employment of sudorifics, that it is impossible with certainty to refer this increased action of the skin in any case exclusively to the nitrate. Dr Basham watched the effect of the nitrate in many cases of acute rheumatism, in which large doses were employed without other remedies, and in which no great increase in the perspirations was noticed; but in which, nevertheless, the symptoms of acute rheumatic inflammation were mitigated in a remarkably short time.

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* Bulletins de l'Academie Royale de Medecine. Tome ix. Considerations generales sur le traitement du rheumatisme articulaire par le Nitrate de Potasse a haute dose.

It cannot be doubted, then, that much larger quantities of saline matters can be endured by the system, without detriment or injury to any of the functions or organs of the body, than has been generally believed; and the uniform beneficial results in numerous closely-watched cases, in which an obvious mitigation of symptoms was apparently obtained through the agency of the nitrate, appears to the author to justify the classification of this salt, among the most certain and least hazardous remedial agents in acute rheumatism.

During the progress of these observations, it became a question of importance to determine if the nitrate of potash could be detected in the blood. As the action of the salt was so moderate upon the excreting functions, it was a fair inference that while the patient was taking the salt, there would be no great difficulty in demonstrating its presence in the blood.

"The case was selected of a man thirty years of age, suffering from an attack of acute rheumatism (the third within four years), affecting the wrists, knees, and ankles. On admission, he was bled experimentally to four ounces, previous to being placed under the usual treatment. This quantity was received into two vessels, one for examining the physical characters of the blood, the other for analysis.

"The first portion was remarkably cupped and buffed, presenting a firm, dense coagulum, with a concave layer of colourless fibrine, with perfect separation of the fibrine and red corpuscles from the serum.

"The second portion was analysed, and gave in 1000 parts—fibrine, 7·04; residual salts, 9·41.

"The nitrate of potash drink—two ounces of nitre in two quarts of cold water, flavoured with lemon-peel—was given in twenty-four hours, and continued for three days. He was again bled to four ounces, and the blood divided into two portions as before. The first portion, set aside for coagulation, gave a firm coagulum, but was not cupped, and only presented a faint layer of colourless fibrine on the surface. The second portion gave, in 1000 parts—fibrine, 5·53; residual salts, 13·56; and amongst the saline residue nitrate of potash was detected.

"He was again bled on the ninth day from admission, and when the symptoms had mitigated; the nitrate of potash having been resumed for two days in consequence of a threatened remission.

"The portion set aside for coagulation presented a perfectly separated clot, but there was no separation of the red corpuscles from the fibrine,—no buffy superficial stratum. The portion analysed gave, in 1000 parts—fibrine, 4·05; residual salts, 14·9. Nitrate of potash was again detected.

"From these analyses it will be seen that the fibrine, in the nine days, fell from 7·04 successively to 5·53 and 4·05; while the residual salts gradually increased from 9·41 to 13·56, and at length to 14·9."—Pp. 7, 8.

Dr Basham infers, that while the internal use of the nitrate of potash assisted to restore the proportion of the saline constituents, the general constitutional treatment, by calomel, opium, antimony, and colchicum, tended to lessen the excess of fibrine; and that by the co-operation of these several remedies the rheumatic condition became controlled, and the patient safely guided to convalescence.

Dr Basham, therefore, intends not, by proposing the employment of nitrate of potash, to supersede the use of other remedies when they are indicated. To these the saline plan of treatment is only an auxiliary; for he is aware, that although excess of fibrine and deficiency of salts have been proved to be the essential proximate condition of inflammatory disease, and although saline medicines used internally may possess a certain influence and controlling power on the separation of the fibrine, yet in the majority of cases the saline medication cannot be supposed to fulfil all the purposes and objects of treatment. The various morbid conditions must be relieved by those remedies which general experience has proved to be most efficient; but in co-operation with them, most valuable aid may be derived from the nitrate of potash; and in no disease has its therapeutic influence been more apparent than in every form of acute rheumatism.

The results of the treatment, as presented in tables at the end of the paper, are the following. Among seventy-nine cases of acute rheumatism, among which were 58 males, 10 suffered from cardiac symptoms before or after admission; and of these, two died. The other twenty-one cases were females, among whom eight suffered under cardiac disease before or after admission; and of these, one died. Among seventy-nine cases, therefore, of acute rheumatism, were 18 instances of cardiac disease and three deaths.

For the fifty-six males who recovered, the average duration of the symptoms from admission to convalescence was 31·10 days. Among the twenty females who recovered, the average duration of the symptoms from admission to convalescence was 34·15 days.

In all these cases other remedies were given; for instance, calomel as a purgative and as an alterative; mercurials in general; calomel and opium; colchicum; Dover's powder; purgatives; local bleeding; bark; warm baths, and the local application of nitre, in solution, by means of spongio-piline.

Dr Basham, further, in opposition to the opinion usually held

among physicians, thinks that the external application of saline substances, and especially that of nitrate of potass, is most useful and important in the treatment of acute rheumatism. In observing and reflecting on the effects of saline medicines internally, he was led, by an observation of Mr Gulliver, in one of the notes to the edition of Hewson's works, to think that their local application might be beneficial. Mr Gulliver there expresses the opinion that the efficacy of saline medicines in inflammation may depend on their correcting the disordered state of the blood, by preventing or destroying the aggregation of the corpuscles, and, consequently, their tendency to separate from the fibrine, and to be accumulated in the minute vessels, and that hence the application of a solution of salt to inflamed parts might prove a very useful and simple remedy.

The only reflection which this passage suggests is, that it appears singular that the idea of local application did not occur to the author without the aid of this observation; for certainly nothing is more common than the application of saline solutions, hot, cold, and tepid, to inflamed parts. One practitioner uses hot salt water, or a solution of strong bay salt; another uses sal-ammoniac; and a third uses acetate of ammonia; all, we grant, upon different speculative grounds, but all with the same object, namely, to abate local inflammation. Some of these applications must, if long continued, be absorbed more or less freely by the surface of the parts. Some, probably, are imbibed through the interstices of the epidermal scales. If they are of less strength than the saline impregnation of the blood, they are taken up by imbibition; if their strength is greater, they must be received by endosmosis, and in this manner mingled with the blood.

It is certain that the alkaline waters of Vichy and Ems are in some manner, either by imbibition or endosmosis, absorbed into the circulation; for, after the use of one or two baths, the urine is decidedly alkaline. It is also certain that the waters of Wiesbaden and other saline waters are absorbed.

It is proper, however, to allow Dr Basham to explain his own views on this point. He could scarcely doubt, he states, after witnessing the effects of nitre taken internally in acute rheumatism, that the impregnation of an inflamed part with saline matters by absorption through the skin, would lessen the intensity, and abridge the course of the inflammatory process, and might often, if not in all cases, prevent the formation of those impediments to motion which constitute the most serious morbid effects of rheumatism affecting the joints.

Here, also, the author assigns the preference, at least in acute rheumatism, to nitre. Iodide of potassium he has frequently used externally in chronic arthritic affections with apparent advantage;

and in a few cases of gout, the bibasic phosphate of soda was employed with manifest relief to the severity of the local symptoms.

Dr Basham, in estimating the true value of this mode of medication, is quite aware of the fact, that, in cases of this class, the local symptoms may either spontaneously undergo abatement, or are liable to move to other parts. From observations made during two years previous to the date of this paper (May 1848), he is convinced that a special remedial agency may be justly claimed for this external saline method of treatment.

"When I first commenced the employment of these saline fomentations, the salt was brought in contact with the skin by means of light flannels, saturated with the saline solution, with which the limb was enveloped, the whole being covered with oiled silk to prevent evaporation. For the hands I have used worsted gloves. On the feet the foot of a worsted stocking, saturated with nitre, and covered with oiled silk.

"But since the invention of Mr Markwick's new epithem, I have found it so admirably adapted for my purpose, that now I never employ anything else. My method of employing it in acute rheumatism is as follows: if the hands, elbows, knees, or feet are the seat of inflammation, gloves or caps are made of the spongio-piline to fit these parts; if a greater extent of surface be involved, a portion of the spongio-piline is cut large enough to envelope the entire surface. The spongy surface of the epithem is first moistened freely with water, and any superabundant fluid squeezed from it, so that the linen or bed of the patient may not be unnecessarily wetted. Nitrate of potash, or the salt to be employed in powder, is then freely and plentifully sprinkled over the moistened surface, or rubbed in to secure its solution, and the thorough impregnation of the epithem; it is then applied to the inflamed part, and lightly secured by a roller. Nothing further is required than once in about six hours to remoisten the spongy surface; fresh addition of nitre is never required if sufficient quantity has been used in the first instance; the salt in powder is hence better than a saturated solution, which nurses seldom succeed in making.

"In numberless instances, by this simple treatment, I have witnessed the most palpable and evident relief to the local inflammation; and many rheumatic patients who have suffered from previous attacks of the disease, and have known relief to their local sufferings only through the instrumentality of the constitutional treatment, have declared that they marvelled at the speedy abatement of the pain, redness, and swelling, for they had never experienced such unexpected relief before.

"As I am anxious to avoid the tedium of reports of cases, I propose in this place to state the result of the experiments adopted to test the efficiency of these saline applications in subduing local rheumatic inflammation. The object was to determine how far the relief obtained or apparent, could be attributed to the agency of warmth and moisture, and how much to the effects of the salt employed. Cases were selected in which both hands or two distant parts were in a state of rheumatic inflammation. Each hand was placed in a glove of spongio-piline, one was merely moistened with hot water, the other saturated with nitrate of potash. In every instance the local inflammation yielded to the saline remedy, while the other hand continued as if no local remedy had been employed. Eight different cases afforded exactly similar results. In five of them the local inflammation was in both hands; in two in both knees; and in one in the right elbow and left wrist.

"In each case the part to which the salt was applied was relieved within twenty-four hours, the other parts continuing inflamed and painful, till they were subjected to a like treatment, notwithstanding the moist fomentations in which they were invested.

"That the relief obtained in these cases could not justly be attributed to the constitutional treatment usually proceeding at the same time, is proved by the fact that during the earliest periods of the treatment, remission of the local symptoms was observed only in those parts to which the salt was applied.

"The following case illustrates this point: A man, aged 45, was placed under treatment, May 11, 1847, suffering from acute rheumatic inflammation of both knee-joints, the elbows, and right wrist. These parts were red, swollen, and painful. He was ordered the usual treatment, but the elbows and right wrist were enveloped in the nitre fomentations by means of the spongio-piline. In twenty-four hours all redness had disappeared, and there was perfect mobility without pain, in both hands and elbows; but the inflamed state of the knees continued unmitigated: these were treated with the saline application, and the next day all swelling had disappeared; the joints could be moved without pain, though he could not stand from debility; but his convalescence proceeded uninterruptedly.

"In cases also of cardiac complication, very marked relief has been obtained by covering the præcordial region and anterior surface of the chest with the spongio-piline saturated with the salt. In several instances the irregular murmurs, particularly those of exocardial and pericardial disease, have disappeared even before the system had exhibited any indication of being under the influence of mercury."—Pp. 10–12.

The most efficient plan, it appears, would be to employ a hot bath, containing a saturated solution of saltpetre.

It is impossible to doubt, that, if the expectations thus held out from the use of nitre in rheumatism external and internal be realised, great service is rendered to practical medicine, and great benefit must accrue to patients. Under all circumstances, Dr Basham is entitled to thanks for the diligent and systematic manner in which he has investigated the true powers of this remedy. His Clinical Report furnishes the most ample and conclusive information extant on the therapeutic effects of saltpetre in acute rheumatism.

It may be proper to mention two circumstances; the first is, that Dr Golding Bird, as we shall see, maintains that acetate of potash is as effectual as any other remedy in the abatement and removal of rheumatic symptoms. Acetate of ammonia has been long in use as a saline diaphoretic in the treatment of rheumatic disorders. It is possible, also, that cream of tartar, the bitartrate of potash, might be as serviceable in many instances as nitre.

Secondly, It deserves remark, that, in one of the formulæ for the compound called the Chelsea pensioner, noted as an empirical and popular remedy for the treatment of subacute and chronic rheumatism, nitre forms one ingredient; while, in the other formula, cream of tartar, the bitartrate; occupies the same place. The first formula is the following: one drachm of guaiacum, in powder or shavings, two drachms of rhubarb in powder, one ounce of nitre, and two ounces of sublimed sulphur, with one nutmeg mixed with as much honey as to form an electuary. In the second formula, which is the most usual, and the one given in many works, the place of the nitre is occupied by one ounce of cream of tartar. In speaking of this compound, we may observe that it more commonly acts as a gentle diuretic than as a laxative; but occasionally it moves the bowels pretty freely. In both formulæ, it may be added, the guaiacum is too small in proportion to the other ingredients to be of much or decided benefit. The amount of that substance ought to be two drachms at least; and in circumstances in which we have found it expedient to prescribe its use, we have thus altered the constituents. In hospital practice, and also among poor patients generally, the honey may be most conveniently dispensed with, and its place occupied by treacle.

The proposition to employ nitre, or saltpetre, in the treatment of rheumatism is a smaller departure from established methods than that brought forward by Dr Owen Rees, to administer lemon-juice. Much of what may be termed the subsidiary treatment of rheumatism during the last twenty years has proceeded on the principle of saturating and neutralizing acidity either in the alimentary canal or in the blood; and hence bicarbonate of soda, bicarbonate of potass, pure potash water, and lime water, have all been given, either along with or after meadow-saffron. That this is the case can be fully shown by the prescriptions of many practitioners in the ordinary periodical works and other publications during the last twenty years. Meadow saffron and bicarbonate of soda, meadow saffron followed by magnesia, and occasionally the use of nitrate or tartrate of soda in the form of effervescing powders, have been, next to calomel and opium, and calomel and laxatives, a favourite method of medication in the management of rheumatic symptoms. It is impossible also to deny that, under this method of management, many instances of rheumatic disorder, acute and sub-acute, recovered.

Dr Owen Rees proceeds on an opposite, or at least a different principle; and disregarding or making little reference to the indication or supposed indication for neutralizing acidity, cures the disease at once by administering lemon-juice.

In the 110th number of the Medical Gazette, Dr Rees directed attention to the beneficial effects produced by the administration of lemon-juice in the treatment of rheumatic diseases.* Since that time the remedy has been used by others as well as by himself, we are informed, with very decided success; and he thinks himself entitled to infer, that it possesses efficacy quite equal to, or even greater than, any of the preparations of meadow saffron. Lemon-juice has the remarkable property of lowering the pulses of persons suffering under acute rheumatism in as great a degree and as speedily as meadow-saffron does; while the early relief from pain is such as is seldom observed to follow the use of the latter drug.

In some instances the amendment under the use of lemon-juice was rapid and uninterrupted. In others, though amendment took place, it was not so speedy, and the disease appeared less amenable to the remedy. Under such circumstances, the treatment by means of lemon-juice was subjected to such modifications as the symptoms seemed to require. Dr Rees, nevertheless, is disposed to think that the progress of the disease, even when most obstinate, will, under the use of this new remedy, be found to bear a very favourable comparison with the history of those cases in which the former methods of treatment have been pursued.

The form of rheumatic disease in which the greatest benefit has been hitherto derived from the use of lemon-juice is that of acute rheumatism, and that variety of rheumatic disorder which involves the small as well as the large joints in acute inflammation, and which, the author explains, is known as rheumatic gout.

On this point it may be observed, that, in Rheumatic Gout, the disorder is not, strictly speaking, acute inflammation. In most instances of this disorder, the symptoms are those which distinguish the sub-acute, or semi-acute, and, in some instances, the chronic form of the disease.

In cases of pure gout, in which the inflammation is high, the author is informed that great advantage has been observed to result from the use of the remedy; in the sub-acute and chronic forms, however, the same benefit has not been observed to follow. In acute rheumatism, also, after inflammation has been abated, and debility remains, its employment is less beneficial than previously.

In chronic rheumatism, some relief follows the administration of lemon-juice; but, as the author allows, that lengthened experience alone can determine the question of the fact and amount of efficacy in a disease so lingering and capricious, it is understood that this point is still under probation.

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* Medical Gazette, Vol. xliii. p. 156. 26th January 1849.

In the present tract, Dr Owen Rees gives the details of eight cases of rheumatism, acute, sub-acute, and sub-acute supervening on the chronic variety, in which, with the use of laxatives, the administration of lemon-juice was followed by speedy disappearance of the symptoms, and recovery on the whole rapid.

Of these eight cases, six are examples of acute articular rheumatisms, with pain and more or less swelling of the joints, mostly the large joints, as the knees, the ankles, the elbows, the wrists,—and heat, and in several instances redness, of the integuments. Of the other two, one is an instance of chronic rheumatism in a man of 36, affecting three or four times annually, during the space of fourteen years, the hips, knees, ankles, toes, shoulders, elbows, wrists, and hands. The other is an instance of subacute rheumatism supervening in chronic rheumatism, and with probable syphilitic complication.

In all these cases, after the exhibition of laxative or cathartic medicine so as to procure several motions, lemon juice was administered along with syrup in water, in such manner and to such amount, that the patient took from one ounce to two ounces in the course of twenty-four hours. In two cases, one ounce was ordered every six hours. In one case, one ounce of the lemon juice was ordered three times daily. At the same time, however, cathartic medicine was exhibited pretty freely. Thus, one scruple of a mixture of rhubarb and calomel appears twice in the second case; and once in the fifth case. The saline rhubarb powder appears twice in the third case. In the fourth case the patient lost blood to the amount of twelve ounces, had one drachm of the compound jalap powder, and a blister on the left knee. In the case with supposed syphilitic complication, the patient took twice twenty minims of the wine of meadow-saffron seeds in a draught of tincture of senna, had iodine tincture rubbed over the affected joints, and took iodide of potassium internally.

The lemon juice was administered in the manner above mentioned for eight days, eleven days, and twelve days, according to its effect on the symptoms. In the third case, that of a young man aged nineteen years, it appears to have been given for twenty or twenty-one days before so decided an impression was made on the disease, as to afford much relief; and the patient, who was one full month or 30 days under treatment, though decidedly relieved at the date of the last report, is still directed to continue the remedy.

The effect of the medicine on the urine is remarkable and decided. It is well known, that in acute rheumatism the urine is scanty, high coloured, of density above average, and most commonly loaded with lithate of ammonia, or lithic acid, or both. When the lemon juice has been administered for several days, from six to eight or ten, the urine becomes more copious, clear, returns to its natural density, and almost constantly shows the acid reaction without indication of the presence of the lithates.

The remedy also appears, so far as a judgment can at present be formed, to moderate the action of the heart, to diminish the frequency of the number of its contractions, and to render them slow, regular, and naturally effected.

Such are the general results of the cases adduced by Dr Rees. To illustrate the particular cases, we shall merely advert to the particulars of two; one of acute rheumatism, the other chronic.

In the first case, a female of 22, the symptoms were with more or less quickness of pulse, pain, swelling, and redness of the large joints, as the knees, ankles, shoulders, and elbows, of a mixture of one ounce of lemon-juice with two drachms of syrup; five drachms were ordered in water three times daily. This was continued for eleven days, at the end of which the swelling and pain were gone, and the pulse, which had been previously less frequent, was steadily at 72. At the end of nineteen days, the lemon-juice was given up, and tincture of sesquichloride of iron was ordered. The case was twenty-seven days in hospital.

In the instance of chronic rheumatism, the patient was a man aged thirty-six, a brazier by occupation, and thereby exposed to great vicissitudes of temperature. After an attack of rheumatism fourteen years previously, he had three or four times annually attacks of pain and stiffness in the joints, varying in severity. When admitted to Guy's Hospital on the 15th May 1849, he had been three weeks ill. He took pure lemon-juice for nine days, without benefit. The joints, in short, became worse. He was in the habit of using lead in large quantities in the course of business; and the usual broad blue line round the gums was distinct He never had either colic or palsy. He was unable to extend the fingers, the joints of which were enlarged, but not painful. The urine was acid, clear, not coagulable, of density 1017. No indication of heart disease was perceptible.

On the 19th, after taking one ounce of lemon-juice three times daily, he was much better, with less pain in the joints. On the 23d, though the joints were weak and stiff, he could walk by the aid of a stick; on the 29th, when he could walk without assistance, he was dismissed as relieved. Thus, in the course o. fourteen days, all the worst symptoms in this man's case subsided under the use of lemon-juice. We infer that the lemon-juice, which, it is stated, he had taken pure, was ordered before his admission to the hospital.

On the probable theory of the mode of operation the following observations are given :—

"The use of Lemon Juice as a remedial agent has of late years been almost restricted to its administration as a cure for scurvy. It formerly would seem to have enjoyed some considerable reputation as an antisyphilitic. We learn from Percival's works that the juice of lemons has been found also of great service in certain biliary and stomach derangements, and, as the experience recorded is not without interest in connexion with the present subject, I shall here quote the author's words. After relating some experiments made on gall, by the addition of lemon-juice and other acids, he proceeds to express an opinion that the beneficial effect is to be traced to its power of 'sweetening putrid acrimony.' The facts recorded are, however, as may be imagined, more valuable than the theory propounded. Our author proceeds—'A tablespoonful of the juice of lemons, unmixed with anything, is said by an ingenious writer (Whytt on Nervous Diseases), to have repeatedly proved a certain cure for a palpitation of the heart, after many of the medicines called antihysteric had been tried in vain. This conjecture is confirmed by a similar case which Dr Bisset hath related, of a middle-aged gentleman who had a palpitation of the heart, accompanied with some symptoms of the jaundice, and who was completely cured by drinking every evening weak rum, acidulated with the juice of Seville oranges.'

"Cases are also quoted by Percival, from which it would appear, that calculous diseases have been greatly benefited by the use of lemon-juice; so much so, indeed, as to have led to a belief that it possessed lithontriptic properties. Sydenham also recommends lemon-juice and manna as a remedy for gravel, and speaks of its great efficacy in his own case. The account of his own sufferings and relief may be found in his works, in the chapter 'de mictu sanguineo a calculo renibus impacto.' He there relates how his nephritic affection formed the sequel to an attack of gout. The probability that the beneficial effects quoted by the authors cited were produced by lemon-juice, is to a certain extent increased by the experience we have had in the treatment of rheumatic diseases: stomach derangement, disturbance of the function of the heart, and calculous nephritis, all bearing a relation to the rheumatic and gouty diatheses.

"As regards the theory of the action of lemon-juice in the cure of rheumatic and gouty disease, I have but little to say. I recently published the view which I was inclined to take on the subject, but am by no means anxious to place it before the profession as anything but an extremely doubtful matter to my own mind; it being rather to the facts contained in the above cases that I earnestly beg attention.

"Citric acid, the chief ingredient in the juice of the lemon, if considered in the anhydrous state, is composed as follows :—

Carbon ....4 atoms
Hyrogen.........2 „
Oxygen.........4 „

" This shows a great excess of oxygen over and above the quantity required to saturate the hydrogen present; and under such conditions it may be considered probable, that when introduced into the stomach this excess of oxygen may there be made use of to modify the results of the digestive process. Knowing, then, as we do, that in the class of gouty and rheumatic affections there is a tendency to the introduction of lithic acid into the circulation, in what manner may we suppose this excess of oxygen to assist us? Let us first consider the constitution of lithic acid, and we shall observe the fact, so well known to animal chemists, that its elements may easily be converted into those of urea and carbonic acid, by the addition of water and oxygen. This will be at once apparent from the following diagram :—

$$\begin{array}{lr|c|lr}
\text{1 atom of lithic acid} & C_{10}\,N_4\,H_4\,O_6 & & \text{2 atoms of urea....} & C_4\,N_4\,H_8\,O_4 \\
\text{4 atoms of water....} & H_4\,O_4 & \text{equal to} & \text{6 atoms of carb. ac.} & C_6\qquad\quad O_1 \\
\text{6 atoms of oxygen...} & O_6 & & & \\
\hline
 & C_{10}\,N_4\,H_8\,O_{16} & & & C_{10}\,N_4\,H_8\,O_{16}
\end{array}$$

" It was reflecting on the exigencies of the case, more especially in reference to the necessity for a supply of oxygen, that the exhibition of lemon-juice in gout and rheumatism suggested itself to my mind; and I moreover considered, that probably the small proportion of alkaline citrate present, which, by decomposition during digestion yields an alkaline carbonate to the blood, might assist in the cure. Whether there be truth or not in this theory matters little, in comparison with the discovery of a remedy, which, I believe, assists in obtaining earlier relief than has heretofore been the case, in a most distressing malady.

" The most marked physiological effect produced by the exhibition of lemon-juice is, perhaps, the great diminution in the action of the heart and the lowering of the pulse. The relief from pain is, however, so early obtained, and so much more complete than by other remedies similarly affecting the pulse, that I am disinclined to attribute it entirely to that quality. I was anxious to ascertain whether we should not observe some effect on the pulse by exhibiting the juice to a healthy person, and one of our clinical clerks, Mr Sanders, immediately volunteered for the experiment. This gentleman took one ounce of the juice three times a day for three days, and carefully noted his pulse, which was naturally full, and 75 in the minute. After five doses the pulse became much weaker and more compressible, and numbered 70 in the minute; conditions accompanied by a feeling of general depression. On the third day the pulse became as low as 66, and was very small and compressible. The urine was always acid, and also natural in quantity till the third day, when it increased somewhat; the specific gravity was then 1017, and there was a deficiency of lithic acid."—Pp. 32–37.

It is important to observe that the urine is never rendered alkaline by the use of the vegetable acid; and in the case in which it was alkaline, that is to say, morbidly alkaline, before treatment, it became acid after administration of the lemon-juice. From these circumstances the author infers that the beneficial results are not to be ascribed to the alkaline citrate (citrate of potass) of the lemon-juice.

Since the experiments of Dr Owen Rees have been made known, other physicians have employed lemon juice in the cure of rheumatism. At a meeting of the Medical Society of London, it was stated by Dr Theophilus Thomson and Mr Middleton, that they had observed the symptoms of acute rheumatism to disappear in a short time, and in a very satisfactory manner, under the use of lemon juice; and Mr M. submitted, as a question worthy of consideration, what might be the method in which the remedy operates. Dr Golding Bird stated his belief, that it was not wonderful; that the uric acid, which is the main cause of mischief in this order of diseases, is removed by various means under the use of citric acid, of opium, of acetate of potass, and similar agents. He, indeed, maintained, if the report can be trusted, that acetate of potash was as good an antirheumatic agent as any thing else. Dr Garrod denies the presence of uric acid in the blood in rheumatism in greater amount than in health.

At a subsequent meeting of the same Society, held on the 24th September 1849,—the first of the season,—the correctness of this doctrine was controverted by another gentleman, Dr Theophilus Thomson, who denied the identity of operation of lemon juice and acetate of potash. In proof of this he adduced the case of a gentleman, who, while taking acetate of potass for the purpose of removing effusion within the pericardium, was attacked with articular rheumatism in a severe form. This increased in severity so long as the acetate of potash was continued. The urine contained a large quantity of lithates. Lemon juice was substituted in place of the acetate of potash; and in the course of a few hours all the rheumatic symptoms disappeared, while the urine became copious and clear.

It may be some time, perhaps, before all the evidence regarding the true influence of lemon juice in the treatment of acute rheumatism is established to the satisfaction of impartial observers. It seems, nevertheless, impossible to doubt, that this article possesses a very considerable degree of power in changing the state of the blood, and that of the secretions formed from the blood.

It is proper to observe, that an American physician has, in the treatment of acute rheumatism, made trial of the use of lemon juice, and found it to give rise to most satisfactory results.

PART III.

MEDICAL INTELLIGENCE.

TOXICOLOGY.

In the abstract of the Memoir on Poisoning by Hydrocyanic Acid by Mr Nunneley, that gentleman refers several times to two cases of poisoning by that agent; one fatal, the other terminating in recovery. That readers may understand the points referred to in the Inquiry, abstracts of these two cases are here given. The first of these cases acquires peculiar importance from its resemblance in several respects to the melancholy case of the late Mr Thomas Morton. In both instances, there were proofs of similar disease of the cerebral membranes; in both there was the same despondency and unsettled state of the mind; and in both, hydrocyanic acid was taken, and produced fatal effects.

Case of Poisoning by Hydrocyanic Acid.—By THOMAS NUNNELEY, Esq., Surgeon to the Leeds Eye and Ear Infirmary, &c. (From the Provincial Medical and Surgical Journal).—On the morning of Monday, the 26th May 1845, between the hours of ten and eleven, probably about 15 or 20 minutes before eleven, a gentleman, engaged in business as the member of a respectable firm in Leeds, walked into the George and Dragon Inn, in the Briggate of that town, entered an apartment on the ground flat, where on a table was an empty glass and a jug containing hot water, laid down his hat on the table, lifted the glass, and went up stairs into an apartment where there was no other person. The noise made by his going up stairs, and opening and shutting the door, attracted the attention of a female servant, the housekeeper, who was at the moment in the bar. This person entered the low room, and observing on the table a hat and the glass removed, imagined that something unusual had taken place. She returned to the bar, and found there Mr Tennant, a gentleman who had entered the house with the expectation of seeing the landlord, Mr Thompson. The landlord not being at home, this gentleman, Mr Tennant, took up the newspaper, and, while reading it, the housekeeper informed him that she found in the low room a gentleman's hat, a drinking-glass missing, and that some person had gone up stairs. Mr Tennant recommended the housekeeper to go up stairs and see who the person was; but this, she said, she dared not do; and as she seemed alarmed, the gentleman, Mr Tennant, under the apprehension that some person had entered the upper apartment for an improper purpose, went himself. About three minutes before this, Mr Tennant, while reading the newspapers in the bar, heard the front door of the house opened, which, having a spring and creaking when pushed up, made a peculiar noise. Mr Tennant, however, did not hear any person go up stairs. When he entered the room, he found a gentleman lying on his back on the sofa, with his hat off, and an empty glass standing on the table. On seeing Mr Tennant enter the room, the gentleman sprung up and sat, and appeared like a person who was suffering from excess of drink. Mr Tennant walked across the room, said he wished to see Mr Thompson, and apologising for intruding, left the apartment; the gentleman all this time sitting on the sofa, but looking stupid, and his eyes rolling in a singular manner. Mr Tennant left the room, with the idea that the gentleman was intoxicated and had lain down on the sofa till he might be more sensible; and this opinion he expressed to the housekeeper on coming down stairs.

As this woman, however, expressed apprehension in consequence of the gentleman, who was known to be a man of very retired habits, taking away the glass, Mr Tennant again went up stairs, when he found the gentleman reclining on the right side, with his head on the sofa cushion. At the remark, "You seem ill, Sir," he made an attempt to speak, but could not articulate intelligibly. Mr Tennant then going close to him, said, "You must excuse me interfering with you, but you seem very ill;" and asked if he should send for a doctor. He answered merely, "No, it is too late; it is too late." These were the only words he uttered.

Mr Tennant instantly went and brought Mr Nunneley, who was at hand. From the time that Mr Tennant heard the street door creak, to that when Mr Nunneley entered the room, not quite fifteen minutes had elapsed. This, according to Mr Nunneley, was about ten minutes before eleven. If so, and if the previous time was noted with accuracy, the interval between the gentleman entering the house and being found in this state, was only ten minutes, or not so long. When Mr Nunneley entered, the gentleman was lying on the sofa on the right side, alive, but quite unable to speak. It was difficult to say whether he was conscious or not; Mr Nunneley thinks that he was not so in the popular sense of the term. Mr Nunneley shook the patient by the shoulder, and shouted in his ear; but whether the latter heard or not he could not say. He seemed for the instant to recognise the voice; but this there were no certain means of ascertaining. The only indication of consciousness was, that when water was dashed on him, which was instantly done, it decidedly roused him.

Mr Nunneley immediately suspected that hydrocyanic acid had been taken, and dashed cold water on the face repeatedly, and administered ammonia. The water had the effect of rousing him and causing him to take a deep inspiration. The ammonia, which was stronger than was believed, excoriated the lips, and its administration was followed by indications of pain; the patient moving the lips as if he desired to eject, and resisting the introduction of the ammonia into his mouth. Ammonia was also freely applied to the chest. The feet and legs were placed in a pail of hot water, containing a quarter of a pound of mustard. As soon as the legs were placed in the depending position, they became very dark in colour, from the blood vessels being so much loaded; but as soon as they were replaced on the sofa, after death, this dark-coloured appearance went off.

The patient gradually became quieter and more feeble. The legs, arms, and chest were slightly convulsed; the upper extremities more than the lower; and the jaw was fixed. The eyes were prominent, staring, and glassy; and the pupils widely dilated. The countenance was distended, or bloated, and dark-coloured, inclining to purple. Some foam surrounded the mouth. He breathed slowly and convulsively, something like violent sobbing. The hands were partially contracted, the fingers rigid, and about the nails of a leaden colour. The pulse was felt beating distinctly but slowly.

The state of symptoms now specified continued for nearly half an hour, it is stated, with gradually increasing weakness; the pupils became then a little contracted; breathing then ceased; and the patient presented no mark of life. Mr Nunneley remained till death took place, and he thinks that fully half an hour elapsed between the time of his first seeing the gentleman and the instant of death; and that three quarters of an hour had elapsed from the time when he had taken the article, which proved fatal, till death.

The odour of hydrocyanic acid was sufficiently distinct to Mr Nunneley's senses that he felt it in the apartment, and before the body was opened. In the left hand coat pocket, opening inside, was a phial or bottle, with the stopper in it, not quite empty. The glass was empty, and had no smell of hydrocyanic acid; and from this Mr Nunneley inferred that he had swallowed below stairs the contents of the phial, and then walked up and thrown himself down on the sofa.

It was then ascertained that on Saturday the 24th of May, between the hours of nine and ten in the morning, the gentleman had called at Mr Moxon's, a druggist in Boar Lane, Leeds, and purchased a one ounce phial full of hydrocyanic acid, and marked "Scheele's strength." He was acquainted with chemistry, was aware of the properties of hydrocyanic acid, and as he was a customer of Mr Moxon, and presented in his manner nothing to indicate anything uncommon, the latter gentleman did not ask any questions, and merely observed that if the purchaser had been a stranger, he, Mr Moxon, would have required a witness. Mr Moxon, in short, felt not the slightest suspicion that the gentleman wanted the acid for an improper purpose.

According to Mr Nunneley, who had attended him at times professionally for nearly four years, Mr A., the deceased, was a very intelligent man, but a person of extremely nervous and excitable temperament, sometimes combined with despondency. It has happened that he had called in the morning to consult Mr Nunneley, and when the latter met him in the street, an hour afterwards, he has passed without taking the smallest notice. He had an attack of fever some years previously; and since that time he has complained of his head. He was a person of quiet, reserved manner, and retiring habits; and when he used to call at the George and Dragon, which he did frequently to see the newspapers, he always went into an empty room, if he knew there was one unoccupied. His usual time for calling was in the morning, so that here even little ground was given for suspicion, except in the facts noticed by the housekeeper. From his desponding character, Mr Nunneley thought that he was a person not unlikely to commit suicide.

The body was examined on the same day, Monday the 26th May, five hours and a half after death, by Mr Nunneley, with the aid of Dr Pyemont Smith, who had come in when the patient was expiring, and in the presence of Mr York and Mr Rowley. The following were the appearances, external and internal.

Body still warm; upper parts pale, but all the depending portions livid; the cutaneous veins, especially those of the legs, filled with fluid blood which could be easily pressed out; but as soon as the finger was removed, the veins became filled again. The limbs about as rigid as usual at the same period, after death. The fingers not discoloured, except round the nails, where they were of a lead colour. Countenance placid, but somewhat turgid. The eyelids closed; eyes prominent and glassy; pupils moderately dilated; jaws contracted, and lips closed. (The body had not been undressed nor "laid out.") There was the odour of hydrocyanic acid in the room near to the body. The degree, however, in which this could be perceived by those present, on applying in succession the nose to the nostrils of the deceased, differed materially; by Dr P. Smith very strongly; by Mr Nunneley very decidedly, but not what could be called strongly; and by the other two gentlemen, scarcely, if at all; indeed, they could not say they did perceive it.

Abdomen.—On opening the peritoneum, the odour of hydrocyanic was instantly perceptible in any part of the room. This proceeded from the stomach; for on applying the nose to different portions of the cavity, as the observer receded from this viscus, the odour became lost. There was nothing unnatural in the appearance of the viscera, except that a portion of the transverse arch of the colon was much contracted. The bladder was about half distended with urine, none having been passed during or after the action of the poison. The small intestines were filled with food. A portion of the ileum was removed for the purpose of ascertaining if any acid had passed into it. The mucous membrane of both the large and small intestines was natural, and no smell could be detected in any part of the canal. The stomach was removed, and its contents, which consisted of rather more than a pint of half-digested matter (he had eaten a hearty breakfast, about two hours and a half before taking the acid), were emptied into a bottle. The matter smelt sour, and the odour of the acid was exceedingly strong. The mucous membrane was covered with thick mucus, and was red and injected; but perhaps this was not more than might arise from the usual turgescence of the vessels during the process of digestion.

Thorax.—The air was so charged with the odour of hydrocyanic acid from the abdomen, that it was impossible to say with certainty if any arose from the cavity of the chest; if any, it was very slight. The lungs did not collapse; they were healthy; the lower portions gorged with fluid blood. The larger bronchial tubes were filled with reddish frothy mucus. There was no effusion within the cavities of the pleura.

The heart was completely distended on both sides with dark fluid blood, as were also all the large vessels, the blood on both sides being exactly alike. A two-ounce bottle full was collected from the left side of the heart and the aorta, and a like quantity taken from the right side and *vena cava ascendens.* There was considerable hypertrophy, without dilatation of the left ventricle. All the valves were healthy. Very little or no effusion into the pericardium.

Head.—The vessels of the head were filled with fluid blood. The arachnoid membrane was considerably thickened from chronic affection. There was serous effusion in the *pia mater*, upon the whole exterior surface of the cerebellum, as well as the cerebrum; serum was effused within the ventricles. As much as could be obtained clear, was collected (about three drachms), and saved for analysis. There was nothing unusual in the brain, which was firm and elastic,—quite as much, indeed, it was thought rather more so, than usual. The air being loaded with hydrocyanic acid from the stomach, we could not say, with certainty, that any was perceptible in the brain, though we were inclined to think there was, in a slight degree.

The contents of the stomach, which contained a good deal of half-digested food, smelt strongly of hydrocyanic acid, and, when analysed, gave decided indications of the presence of that substance. These contents yielded, after due washing and drying, forty-five grains of cyanide of silver, or 1·8 grains for the whole, indicating 0·4 grain, or four-tenths of one grain, of pure hydrocyanic acid, or twenty-seven grains of medicinal acid of 1·5 per cent. strength, which was found to be that of the acid remaining in the bottle taken from the pocket of the sufferer. From the cyanide of silver, furnished by one eighth of the liquid, Mr West obtained cyanogen gas, burning with its peculiar purple flame.

The fluid taken from the ileum when neutralized, precipitated nitrate of silver, both when suspended over it, and after distillation, but it did not yield Prussian blue.

The arterial blood, and the fluid from the ventricles of the brain, affected nitrate of silver in the same manner. These two fluids, distilled, did not yield Prussian blue.

Mr West, therefore, inferred that the fluids from the ileum, and the ventricles of the brain, and the arterial blood contained a distinct though slight trace of hydrocyanic acid. The venous blood gave no trace.

It is a remarkable fact, in this chemical analysis, that a portion of the contents of the stomach, distilled twenty-three days after the poison had been taken, gave not only the characteristic smell, but a precipitate with nitrate of silver, and furnished Prussian blue precipitate. These facts are at variance with the commonly-received notion, that hydrocyanic acid undergoes rapid decomposition, unless some new acid had been formed from the animal matter,—a supposition scarcely admissible, considering the careful manner in which the analysis was conducted.

From the account of the appearances found in the brain, as well as from the phenomena presented by the manner of the patient during life, it is clear that this gentleman had been labouring for some time under chronic inflammation of the membrane of the brain, at least under *arachnitis;* and that this was the cause both of the uneasy feelings in his head of which he frequently complained, and of the gloomy and despondent state of mind which led him to commit suicide. The gentleman was, in truth, insane. In this circumstance, this case bears a close resemblance to that of the late Mr Thomas Morton.

The quantity of hydrocyanic acid employed in this instance was five drachms at least, probably five drachms and a half. Only two drachms and a half were left in the phial, when it was found. The analysis shows the strength of the acid in the different preparations. This was what is called "Scheele's strength" by the manufacturing chemists. Ten grains of "Scheele's strength" had been consumed by the patient, according to Mr Nunneley, who, however, appears to think that this contained in the phial was not "Scheele's strength." From the contents of the stomach only half a grain of pure anhydrous hydrocyanic acid was obtained.

In this case, no shriek, or cry, supposed and generally represented to denote the peculiar action of hydrocyanic acid on the muscles of respiration, was heard; and none, it is inferred, had taken place. It may be seen that Mr Nunneley questions the constancy of the occurrence of this symptom, and considers it unsafe to place on it any reliance.

The appearance of the eyes is, next to the smell of the acid, as a symptom, the most characteristic; the prominent glassy state of the ball, and the dilated pupil, were as decided in this case as Mr N. uniformly witnessed in animals; but the variation in the degree of dilatation was most curious, and did not terminate with death. At first the pupils were very widely dilated; before life altogether ceased they became rather contracted than otherwise; a few hours after death, when the examination of the body was made, they were more dilated; and on the following day, twenty-seven hours after death, they were still more dilated, but not to the extent they were when Mr Nunneley was first called to him. At this period, twenty-seven hours after death, the eyes were altogether different from what he ever saw in any other corpse; though not so prominent as during the action of the poison, they were more so than usually found even during life; they were clear and bright; the cornea was perfectly distended and glistening, and, as has been said, the pupils dilated. There was nothing cadaveric in their appearance; except the fixedness, they were more brilliant and animated than during life; indeed, this was so marked, that a son and a relative of the deceased, who were with me, were much impressed with it. The rigidity of the body at this time was, as after death under ordinary circumstances.

It has been stated that decomposition of the body speedily comes on; this is possible, though I doubt whether it does so. In this case it did not. There was not the least perceptible change at the time mentioned. The body was not interred until Friday, the fifth day after death. Nothing unusual was observed, but as it was summer, and the friends were anxious, some packets of dry chloride of lime had on the second day been placed in the coffin with the corpse. This may have retarded putrefaction.

Besides the use of cold affusion and ammonia, chlorine was employed as an antidote or means of decomposing the hydrocyanic acid. It seems very doubtful whether the latter agent can be of any service, considering the speedy manner in which the hydrocyanic acid acts, and that chlorine gas itself may cause asphyxia.

Two other remedies have been mentioned, as proper in cases of poisoning by prussic acid—electricity and bleeding. The first, did circumstances allow, Mr N. would certainly be disposed to try; but it is obvious the cases must very rarely occur where opportunity will be afforded for its application, except where everything is arranged, as in an experiment, before hand. The time required for procuring and setting in action either galvanic or electrical apparatus, must, unless in most fortuitous circumstances, preclude it from being relied upon in practice. Bleeding is a means upon which, in the absence of direct experiments, he set not any great value. So far as we possess information of the *modus operandi* of the poison and the symptoms produced by it, venesection is not indicated. The only conceivable means in which the abstraction of blood could be useful would, he thinks, be by relieving the engorgement of the heart, and thus possibly facilitating its propulsive powers. But it must be borne in mind, that the action of the poison is upon the nervous system. Its effects are in some instances too instantaneous to act upon any other tissue, and there is little difference in the effect, whether swallowed, or applied upon any membrane or abraded surface; and the *post-mortem* appearances are rather of a negative than positive character, so far as any structural lesion is concerned. That the heart is not primarily affected is certain, from its being almost if not the very last organ which ceases to show signs of vitality. The length of time which it continues to pulsate, after every other sign of life is fled, is greater in death resulting from prussic acid, than from any other cause with which Mr N. is acquainted. Its pulsations are weak, feeble, and slow, indicating that the stagnation of the blood is the result of want of propulsive power in the heart itself, and not that the loss of power of emptying its cavities depends upon mechanical obstruction to the flow of blood and consequent paralysis from over-distension. In asphyxia the cause of the engorgement of the heart is altogether different: here there is a mechanical impediment to the flow of blood through the capillaries of the lungs, which the heart is unable to overcome, hence the distension of its walls and their inability to propel their contents. The engorgement is mechanical and secondary; the most direct means of overcoming which, is obviously to lessen the force to be overcome by abstracting so much blood, (of course at the same time altering the condition of that which remains, by exposing it to the air,) as shall enable the heart to contract upon and expel that which remains. But if the view which Mr N. takes be correct, the condition of the heart under the influence of the hydrocyanic acid is altogether different; here the distension is primary, from want of propulsive power. This, it is highly probable, is subsequently aggravated by change in the blood, and as well as by the want of change in the lungs, the respiratory function being also impeded: venesection might possibly relieve this latter to some extent, but I do not perceive how it can necessarily affect the former.

Hence Mr N. is not disposed in the first instance to bleed, but should do so after the system had become roused, in order to remove the load of blood, and prevent after-congestion.

Of all the remedies I am acquainted with, says Mr N., I am disposed to place most reliance upon cold affusion. In this instance, more effect was produced by the dashing of cold water than by any other means. Some years ago I well recollect taking a very large Newfoundland dog to the side of a pond, and giving him upwards of a drachm of hydrocyanic acid, originally of Scheele's strength, but having been kept some time, it had probably lost something of its strength; yet, I am convinced, was sufficiently potent to destroy life. He almost immediately began to stagger, when two of us seized him by the legs and threw him into the water, in which he swam about for a time, and presently came out, not much the worse for the dose. Should I have to treat another case of poisoning by hydrocyanic acid, my first step would be to strip the body entirely, and practise cold affusion over the whole body, with the exception of the feet, which I would place in hot water; administer ammonia and chlorine;—then, when it could be swallowed, a sulphate of zinc emetic.

The persistence of hydrocyanic acid is of much importance, and is altogether opposed to the opinions of our best toxicologists, who state that after a few days all traces of the poison disappear, it undergoing decomposition. Yet, in the case now detailed, the acid is found twenty-three days after death without any perceptible alteration in its strength, although placed, as we should suppose, in the most unfavourable circumstances for preservation, being mixed with a mass of partially digested food; the weather being sultry; and no pains taken to ensure its preservation. It is obvious the statements made in books, that the fact of not finding hydrocyanic acid eight or ten days after death has taken place, affords no proof that death has not been occasioned by this agent, must be materially modified. The not finding it even after a much longer period will not be considered as presumptive evidence of death *not* having been occasioned by it.

The only other point to which Mr N. directs attention is the ignorance of druggists, and the different strength of drugs.

Here a druggist states that, not only does he not know the strength of the ordinary preparations of hydrocyanic acid, but that, in the compounding of prescriptions, he is constantly in the habit of using a preparation containing two and a half times the quantity of a deadly drug of that which the prescriber intended to be employed.

But another point of even more importance,—for this ignorance and carelessness might belong only to the individual,—is the fact of an unopened bottle of hydrocyanic acid being labelled Scheele's acid, which ought to contain five per cent. of real acid, and yet on analysis is found to contain only one and a half per cent. Such uncertainty spreads far and wide. For this the manufacturing chemist deserves the severest censure; against such errors the medical man has no means of protecting himself or patient. Suppose a person has been taking prussic acid for some time; the effect has not answered expectation, and the dose has been gradually increased, until perceptible effects are evinced, and they had given acid, supposed to be Scheele's, but in reality only containing a fourth part of it. He then has the medicine made up elsewhere, or the druggist gets a fresh supply of the acid, which is of the correct strength; it is obvious that nearly four times the contemplated quantity would be swallowed, and if the previous dose had been carried to the extent prudence dictated, death would probably be the result,—nature, the drug, or the doctor bearing the blame, while the manslaughter, for such, to speak mildly, it deserves to be considered, would rest upon the ignorance or carelessness of the chemist or druggist.

2. *Case of Poisoning by Prussic Acid; Recovery.* By THOMAS NUNNELEY, Esq. (Provincial Med. and Surg. Journal, No. 33, vol. ii.)—Late on Monday night, 11th August 1845, I was sent for to Kirkstall to see a young gentleman who had been taken seriously ill. When I got there, about half an hour after midnight, I was informed that shortly after the messenger had been sent for me, a man who resided at the next house had taken prussic acid; that Mr Bishop, who is a very respectable young medical man, recently settled in the village, had shortly after seen him, and had acted so promptly and judiciously that he was now coming round. I was, however, requested by Mr Bishop to see the man. I found him in bed perfectly sensible, the limbs natural, the pulse about 60, the heart pulsating regularly but somewhat laboriously; the eyes not unnatural, but injected, perhaps a little prominent, and the pupils rather dilated. He was able to give me a detailed and perfectly natural account of the whole transaction, and of his feelings to the time of becoming insensible. This he repeated to me the next day, and the different parts of it were so confirmed by different persons, that there cannot be any doubt of its accuracy.

The party is about 40 years of age, a book-keeper in a large iron-forge establishment, and by no means one of the most abstemious men. I learned that during the afternoon and evening he had taken not much less than three pints of beer, and at least one glass of brandy; but all agree that he was not intoxicated; and it appears this quantity is not more than is usually taken by him. He had read the account of Tawell's trial, and had been so much interested by hearing that a great number of dogs had been poisoned by prussic acid, that he thought he would give some to a cat and to a rabbit. Accordingly he got three-pennyworth during the evening from a druggist in the village, with this pretence, for such I regard it, although he still says that he had intended it for that purpose; but considering the cool manner in which he took it himself, and the fact of his wife having often been afraid of his destroying himself, there can be little doubt of self-destruction being at the time in his thoughts.

About half-past eleven o'clock his wife retired to bed, in which there was a little girl, five or six years old, already placed. The man, after some hesitation, left the room, and went into an adjoining one; he was followed by his wife, who tells me that she had the idea he was about to cut his throat with a razor; of this she accused him, when he promised her that he would neither harm himself nor any one else, but get quietly into bed.

This satisfied her, and she lay down again, being followed into the room by him. He then undressed himself by the bed-side; when quite ready for getting into it, he passed round to the other side where his daughter was, kissed her, and told her she would soon not have any father, returned to his own side of the bed, and nearly filled a wine glass with common gin; emptied the bottle containing the hydrocyanic acid into the glass, corked the bottle, and laid it down upon the chimney-piece (which was very near to the bed), drank off the contents of the glass which he set down then, threw down the bed-clothes, and got into it, covering himself up with them. He now said to his wife, "Well, Bessy, I have taken something, and it will be all over with me in a few minutes." Though awake, she had not noticed his taking the acid, but this speech, in conjunction with her previous suspicions, alarmed her; she immediately jumped out of bed, and ran screaming to the door. When she left the bed, the child, who was frightened, crept to its father, and said to him, as he informs me, for no one else was in the room, "Father, don't leave me," to which he replied, putting his arms round it, "No, my dear, I won't leave you." He now perceived his jaws becoming stiff, and crammed the sheet between his teeth, thinking, he says, he should like something to hold by. He felt his jaws becoming stiffer and tighter, which is all he was conscious of until the water was dashed in his face, when he was roused, and to his surprise found several persons round the bed. He had no idea of how long he had been in the unconscious state; and says there was not any pain until the water was dashed upon his face; the sensation he then felt, he says, was most awful, but he cannot describe it.

His wife remained out of the room until a man came to her assistance, which, as he was at some little distance, I conclude to have been about two minutes from the time the poison was taken; certainly not less, and perhaps more. They entered the room together, and she states her husband was then lying upon his back insensible, breathing hard, the eyes wide open and staring. the face "black red." The person who entered with her says, he was lying as though in a fit, on seeing which he immediately started off for Mr Bishop, who returned with him, and reached the house, as nearly as can be estimated, in ten minutes after the poison had been swallowed. Mr Bishop informs me, that at this time he was foaming at the mouth; the face was rather pale than otherwise, and that it was covered with large drops of sweat; the arms were rigid, the hands being half extended. He tried to administer some solution of sulphate of zinc, but could not, owing to the jaws being so firmly closed. Mr Bishop then dashed cold water over the head and neck immediately; with the first dash, the man raised himself, and became sensible. Ammonia was now rubbed over the hands, the smell of which he quickly perceived; and tells me he felt better for it, and the sulphate of zinc given; vomiting speedily followed, the matter ejected smelling strongly of hydrocyanic acid. About a pint of blood was next taken from the arm. In this I could not detect any smell of hydrocyanic acid. Mr Bishop, however, said that he thought the blood, when first abstracted, did give off the odour of the acid, but as this was by the side of the bed where the man lay, and had just vomited, I am inclined to think in reality the odour arose from this, rather than from the blood. The blood was of a muddy red brick cast; the following day it had coagulated as usual; perhaps the crassamentum was not quite so firm as usual, but still distinctly separated from the serum. Mr West, who analysed the blood, informs me, that after using every care, he did not find the least trace of hydrocyanic acid in it.

The quantity of acid taken was forty minims. The bottle, a half-ounce one, was quite empty, with the cork in it, upon the chimney-piece. On asking the man the quantity he had taken, he pointed with his finger to the place in the phial to which the acid reached; and on inquiring of the druggist who had sold it the quantity he had given, he said he had not measured it. but he pointed with his finger to the exact spot the man had previously, without his knowledge, done, to show how far he had filled the bottle. On measuring, it was found to contain exactly forty minims, which quantity there can be no doubt was swallowed. The druggist's acid was labelled Scheele's, which he believed it to be; but knowing from the case which had recently occurred to me, and which is recorded in the Journal of July 30th, that this was not a guarantee of the actual strength, I requested to be furnished with some of it, in order to try its effects upon dogs, as well as to obtain Mr West's analysis of it. I ascertained that twenty minims were sufficient to destroy dogs. A small half-grown mongrel dog was dead in two minutes and a half after having swallowed twenty minims; and a full-sized terrier bitch in one minute after the same quantity had been given. Since then Mr West has found the actual strength of the acid to be three and a quarter per cent. of real acid. The man had, therefore, taken nearly one grain and a half of real anhydrous prussic acid, which is probably as large a quantity, if not larger, than any recorded as having been taken and recovered from.

This case, like the previous one, is sufficient to show the continuance of perfect consciousness for some time after the poison has been taken. It might be worth while to inquire what share the remedies had in restoring the man, and what value these or other means possess. But I forbear to enter upon this at present, as, from an extended series of experiments which I instituted, and intend shortly to lay before the profession, I shall be in a better position to show not only the quantity of hydrocyanic acid which is required to destroy life, as well as the effects of it in different doses, but also to ascertain the value and importance of different remedies.

MISCELLANEOUS.

During the last quarter several deaths have taken place in persons under or during the use of chloroform. It is not easy to say whether these fatal results are to be ascribed to the influence of that agent or not; and we decline giving any opinion. But it is important that the authentic facts of these cases, so far as they can be obtained, should be known to the profession.

1. *Instance of Sudden Death from the use of Chloroform. Reported to the Academie de Medecine,* &c. (Gazette Medicale de Paris, 20th Oct. 1849).—Madame Labrune, thirty-three years of age, in good health, applied to M. de Confevron with the request that he would administer chloroform to her previously to the extraction of a tooth, which the dentist stated would be effected with some difficulty.

The patient having been etherized on former occasions for slight surgical operations, by the reporter, without any ill consequences, and being at this time in perfect health, M. de Confevron yielded to her solicitations, not regarding some slight mental emotion which she had shortly before experienced as presenting a sufficient obstacle to the gratification of her wishes.

Having determined to produce only the slightest degree of insensibility, about fifteen grains (one gramme) of chloroform was poured upon a fold of lint the size of a filbert, enclosed in a handkerchief. This was held at a distance from the nostrils by the patient herself.

Its effects were manifested in eight seconds, and the reporter remarked constant winking of the eyelids. The patient repulsed the dentist's hand, making signs that the effect was not complete. She then made four or five fuller inspirations. At that instant, M. de Confevron removed the handkerchief, and only took his eyes from the patient for one moment, occupied by placing it on the nearest piece of furniture; but in this short interval he found the patient's face turned pale, the lips discoloured, her features altered, the eyes turned upwards, the pupils widely dilated, the jaw closed, the head drawn backwards, the pulse not to be felt, the limbs all relaxed; and a few inspirations, at long intervals, were the only remaining indications of life.

For two hours every means of restoration were employed: stimulation of the nostrils by ammonia; frictions of the surface; actual cauterization of the præcordial region; artificial respiration, and galvanism,—all were had recourse to, but without success. The patient was dead.

An examination of the body was made thirty-eight hours after death. The membranes of the brain, and especially the veins of the base of the cranium, were gorged with black fluid blood. The sinuses of the *dura mater* were also full of blood. The substance of the brain was healthy, and when cut presented very numerous dark bloody points. A considerable quantity of serum filled the base of the cranium and vertebral canal. A perceptible quantity of air bubbles was seen in all the veins at the base of the brain. The heart was flaccid, and a considerable portion of black fluid blood mixed with air bells escaped from a puncture made into the left auricle. Air bells were also found in all the large veins of the body. The lungs were crepitant throughout, and presented, when cut, a greyish slate colour. The abdomen was distended by gases. The intestines were not examined.

Remarks.—This case, possessing of itself a high degree of interest, might be regarded as an isolated fact,—as one of those unfortunate cases occasionally occurring, but from which science derives little instruction. It might be said to be one of those rare and inexplicable instances of sudden death from unknown causes, of which other examples may be found in both ancient and modern writers. But viewed, on the other hand, in connection with the cases related by MM. Robert, Barrier, and Gorrè, it acquires great importance, and leads to very different conclusions. It may be observed, in the first place, that facts of this kind have become so multiplied that it is no longer possible to attribute them to any other cause than the operation of chloroform. Omitting all the cases of which the exact details are not given, and confining attention to those already referred to, it is clearly impossible to arrive at any other conclusion. In all these, the symptoms which preceded death, compared with the necroscopic results, prove the extinction of life to have been owing to a real asphyxia, the direct effect of the special deleterious influence of chloroform on the brain.

In the present instance the patient died as if struck by lightning, despite the small quantity of the vapour inhaled, and the precautions observed. There was no warning, as in M. Gorrè's case, no complaint of sense of suffocation; on the contrary, the patient, at the moment of expiring, indicated that the anæsthesia was not complete, and this was shown by her still tightly holding the handkerchief when taken from her.

M. Sedillot has shown, that the supervention of muscular relaxation is the period at which the administration of the agent should cease; but the preceding case shows that this indication is fallacious. The pulse does not furnish a more certain indication, since, in M. Barrier's case, life and the pulse ceased simultaneously.

The presence of air in the vessels, M. de Confevron is disposed to attribute rather to his own forcible efforts to produce artificial respiration, than to the spontaneous development of air in the veins, as a peculiar effect of *ether*, as supposed by M. Gorrè. The other pathological appearances were distinctly those of asphyxia, resulting from a poisonous influence exerted on the brain.

2. *Instance of Death from the use of Chloroform administered during a Surgical Operation.* By Mr SAMUEL SOLLY, *Surgeon to St. Thomas's Hospital.* (Medical Gazette, No. 1144. Vol. xliv. 2d Nov. 1849, p. 757.)—John Shorter, aged 48, a porter, known to Mr Solly for some time as a very active messenger, habits intemperate, but apparently in perfect health, was admitted into George's ward, under Mr Solly, on the 9th October 1849, suffering from onychia of the left great toe, which had existed some time. It was determined to remove the nail, the man having decided before entering the hospital on taking chloroform.

On Wednesday, Oct. 10, at a quarter to 2 P.M., he began to inhale the chloroform with one drachm in the inhaler. It had no visible effect for about two minutes; it then excited him, and the instrument was removed from his mouth, and about ten drops more were added; he then almost immediately became insensible; the chloroform was taken away, and the nail removed. He continued insensible; and, his face becoming dark, the pulse small, quick, but regular, respiration laborious, his neckerchief was removed, and the chest exposed to fresh air from a window close to the bed; cold water was dashed in his face, the chest rubbed, and ammonia applied to the nose. After struggling for about a minute, he became still, the skin cold, pulse scarcely perceptible, and soon ceased to be felt at the wrist; respiration became slow, and at intervals, but continued a few seconds after the cessation of the pulse. Immediately on the appearance of these symptoms, artificial respiration was commenced by depressing the ribs with the hands, and then allowing them to rise again until the proper apparatus was brought, when respiration was kept up by means of the trachea-tube and bellows, and oxygen gas introduced into the lungs by the same means. Galvanism was also applied through the heart and diaphragm, but all signs of life ceased about six or seven minutes after the commencement of inhalation. These means were persisted in until a quarter past 3, but to no purpose. On removing the inhaler, the sponge, which only contains one drachm, fell upon the floor, and the chloroform splashed about,—thus showing that a considerable part of the chloroform remained unused; so that the patient could not have inhaled more than a drachm. Every endeavour was made to procure a post-mortem examination, but in vain.

3. *Instance of fatal termination of Delirium Tremens during the administration of Chloroform.*—(London Medical Gazette, vol. xxiv., Nov. 23, 1849.)—On Saturday, the 20th of October 1849, Mr Joseph Teale, surgeon in Leeds, was consulted by Robert Mitchell, aged 43, an omnibus and cabriolet driver, residing in Hall's Court, Hunslet Lane, Leeds.

Robert Mitchell was a man of intemperate habits, and the previous year, 1848, he had a serious attack of *delirium tremens*, from which however he recovered under the treatment of Mr Joseph Teale. Subsequent to this recovery, he had been induced to abstain from the use of all intoxicating drinks; but this abstinence was only temporary. About two weeks previous to the 20th of October he had reverted to his former habits, and, having indulged in excessive drinking, symptoms of *delirium tremens* again came on.

He had been ill for two or three days, and on Saturday, the 20th, in company with his wife, he called on Mr Teale, presenting manifest symptoms of the disease. Mr Teale prescribed, and on Monday, the 22d October, when Robert Mitchell called again, and was seen by Mr Teale, the symptoms were still present, though, it is stated, not more aggravated. Mr Teale gave him medicine, and the following day he saw the man at his own house. Robert Mitchell was then in bed, speaking incoherently on most subjects, yet sensibly on some. To Mr Teale he seemed worse; but that gentleman thought it improper to place the patient under restraint. The symptoms proceeded; and on the following morning (24th), when seen, he was still in bed; the symptoms of *delirium tremens* were more confirmed. Mr Teale still avoided putting the man under restraint; but expressed his desire that the patient should be watched, and that assistance should be at hand.

Mr Teale saw the patient again on the morning of Thursday, the 25th October, at patient's residence, when he observed less strength in the pulse. The patient was quite manageable. Mr Teale saw the patient again in the evening, about fifteen minutes before 8, at the court-house, it is stated, in a state of great exhaustion. Mr Teale ordered the man to go home. He did so, and undressing himself, by Mr Teale's assistance, got into bed with some persuasion. He wished first to take charge of his horses. When the patient got into bed, the pulse felt so feeble that Mr Teale apprehended the approach of fatal sinking.

Under these circumstances, and finding that all other means had failed in producing abatement of the symptoms, Mr Teale resolved on the administration of chloroform. A blister had been previously applied on the back of the neck, and opium and morphia had been exhibited in adequate doses. Mr Teale poured upon a towel doubled in four, about one tea-spoonful of chloroform, which had been obtained from Dublin, and this he applied by degrees to the nose and mouth. This application was continued about two minutes. The immediate effect was to render the patient calm; and the pulse was gradually improved. This improvement continued four or five minutes; after which Mr Teale waited a little. The patient became more violent, and as his violence increased, the pulse decreased in strength. He had a spasmodic affection or twitching of the face, neck, and arms. Mr Teale applied the chloroform again with the same results as at first. Mr Teale waited until the effects of this second administration had been produced; and then, with assistance, he got the patient raised in bed, while he swallowed some brandy and water. Between these two applications handcuffs were employed, as the man was violent and of great strength.

Mr Teale thinks that chloroform was not administered a third time; but the second dose he allows might be administered at two short intervals. Mr Teale remained three quarters of an hour; at the end of which time the patient was more quiet, and the pulse was stronger. Mr Teale then left the patient, with directions that he should be closely watched, and promising to visit him again in two hours. Death took place before the lapse of this time, that is, on the evening of Thursday the 25th October.

It is proper to observe that the patient is stated, previous to the attach of *delirium tremens*, to have been labouring under bronchitis.

It was thought proper to make this death the subject of a coroner's inquest, and an inquest was accordingly held on the 27th.

Inspection of the body on the 26th disclosed the following appearances. The face and head were enormously swollen, and of dark colour; and vesication had formed on the chest, arms, body, and lower extremities.

A large quantity of dark-coloured serous fluid flowed from the scalp when it was divided.

The brain is described as decomposed, pulpy, and brown-coloured, particularly at the base. In the lateral ventricles was no unusual appearance; no effusion, no clot of blood, no rupture.

The lungs were dark-coloured, congested, and collapsed; the superior lobe of the right lung was attached by old pseudomembrane to the pleura; and in the left lung was an adhering false membrane more recent.

No fluid was effused within the pericardium. The heart was small in proportion to the size of the man. The substance was flaccid, pale, and the chambers empty. The coating of the heart, and the valves of the large vessels were in a natural state.

The mucous membrane of the larynx and trachea was of a reddish brown colour, and much congested; and this colour was traced to a division of each side of the chest.

The liver was healthy; the gall-bladder full of bile. The stomach was empty, and presented no unnatural appearance. The other abdominal viscera were healthy; and much fat was observed in the omentum and mesentery.

On this inquest three medical practitioners gave their opinions.

Mr Samuel Smith, surgeon to the Leeds Infirmary, allowed that the appearances did not account for the fatal result; and he could not give a decided opinion as to the cause of death. He thought that the appearances of the tracheo-bronchial mucous membrane might be produced by the chloroform, and they were not such as he would have expected in a case of *delirium tremens.* He would not as a general rule administer chloroform in *delirium tremens;* but after hearing the account given by Mr Teale of the symptoms, and the inefficiency of other means, he thought Mr Teale was justified in employing chloroform as a remedy. "If I was to give my opinion, I should say the man has died from a combination of *delirium tremens* and chloroform. I should not like to say, that the man died of chloroform; but I think it probable that the man might not at this time have died of *delirium tremens,* if it had not been administered. If I was giving a description of the cause of death for registration, I should say he died of *delirium tremens,* giving the number of days he was under treatment."

Mr William Price, surgeon, considered the appearances in the windpipe, lungs, and heart, the same as those observed in fatal cases from the use of too large a quantity of chloroform. He would not use chloroform in *delirium tremens* without consultation, and then he might be led to employ it. He was inclined to think, that if chloroform failed to effect the relief which it was expected to do, it might, in combination with *delirium tremens,* have caused death. If he were to give a certificate to the registrar, he should say that he, Mitchell, had died of *delirium tremens* as the principal disease; and he thought that if chloroform had not been administered, the patient might have lived only a few hours longer.

Mr Samuel Hey of Leeds, surgeon, was of opinion, that in such circumstances as those of the case of Robert Mitchell, chloroform was properly administered; and the practice of Mr Teale was such as he should have adopted. In another case of *delirium tremens,* quite as violent as this, Mr Hey administered chloroform, though the patient resisted; the man slept twelve hours, and recovery took place.

The Coroner, in summing up, allowed that no blame was attached to Mr Teale, or to any other person; and that the only safe and proper conclusion which the jury could form, was, that the man died of *delirium tremens.*

The foreman, after the jury had conferred together for a short time, stated that they were unanimously of opinion that the man had died of *delirium tremens;* and that it was right to acknowledge that Mr Teale had employed the proper and necessary remedies.

4. *Case of Death ascribed to Inhalation of Chloroform.*—In the Hereford Journal is recorded a death from the use of chloroform, previous to the performance of a surgical operation. On Wednesday the 5th of December, a young Welch woman named Jones, living near the Craven Arms, between Church Stretton and Ludlow, had come to Shrewsbury to undergo the operation of extirpation of the eye-ball. Mr W. J. Clement, the surgeon under whose care she was, administered previously about one-third of the quantity of chloroform, it is said, which he was in the habit of giving to other patients. The patient very soon became insensible, and, after muttering a few words in Welch, expired almost immediately, as if she had been poisoned by hydrocyanic acid.

On the evening of the same day, the coroner held an inquest on the body; and the jury returned a verdict of "Died by apoplexy, in consequence of inhaling one drachm of chloroform."

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Clinical Contributions to the Pathology, Diagnosis, and Treatment of certain Chronic Diseases of the Heart.* By Charles Ritchie, M.D., late Senior Physician to the Royal Infirmary, Glasgow. (Continued from page 33.)

I. *Simple Congestive or Dextral Disease of the Heart.*

In the preceding portions of these contributions an attempt has been made to treat of certain chronic lesions of the heart, under the respective categories of simple congestive or dextral, and of structural or sinistral disease of that organ. The former of these general divisions has been already considered in its subordinate aspects of the earlier, the transition, and the concluding periods of dextral cardiac malady, and I propose, now, to close the general description of this division, with some illustrations of the numerous complications of this form of disease, and a few observations on its treatment. And first of

D, Its complications.

a, With cartilaginous thickening of tricuspid valve.

Case 20.—*Enlargement, and some hypertrophy of heart, succeeded by an acute attack of bronchitis, pulmonary engorgement, and pleurisy, in which, after death, besides the other usual alterations in the viscera, the tricuspid valve was found thickened.*

Hugh Macallum, aged 25, a labourer. This man was admitted into the house a month ago with severe cough, dyspnœa, and pain over all the right side of the chest, increased on making a long inspiration. He then was treated by blood-letting, general and local, and had mercury and antimony. Six days ago he left the hospital nearly cured; but, becoming again ill, he was readmitted. He now coughs considerably, breathes quickly, lies on his right side, in which he has pain on inspiration. Sputum mucous and glairy; skin hot and dry; expression dull, and eyes suffused; pulse full, quick, and moderately firm; tongue whitish, bowels lax; dozes much, and complains of headach. Percussion of right thorax dull, and pervaded by moist crepitus and mucous rattles; behind, the voice is unusually resonant. There is some moist crepitation anteriorly in left thorax.

The former treatment was resumed. The blood detracted on two occasions was in one covered by a thick loose buffy coat, but not cupped; and in the other, the blood remained liquid, but was covered by a loose albuminous coagulum. He died on the 7th day.

Inspection.—Both lungs were partially adherent to the thoracic parietes, and their texture filled with bloody serum, which flowed out on pressure. Numerous portions were solidified by hæmoptysical engorgement. The bronchial lining was intensely red, and the tubes blocked up with mucus. The heart was large, and the right auriculo-ventricular valves were cartilaginous. All the walls were slightly hypertrophied. The pericardium contained about three ounces of serum.

Some similar effusion had taken place within the abdomen. The liver was of natural size and colour, but presented much of the tuberculated appearance. The spleen was twice its usual bulk, and very pulpy. The kidneys were much congested and firm.

It may be said that the alteration of structure in the tricuspid valve in this man was less a result of endocardial inflammation produced by causes emanating from the lungs, than of an extension of disease from the liver. I think I have met with examples of osseous degeneration of this valve from the latter cause. But the enlarged condition of the heart, in the case now reported, proves that in it there was an amount of previous simple cardiac distension of which no notice was taken in the report, and renders it probable that the changes of structure in the liver, which are also vaguely stated, were products likewise of an anterior chronic bronchial affection.

I have frequently, in my dissections of subjects who have died from dextral disease of the heart, met with strongly marked traces of exocardial inflammation of the right auricle, the terminal nature of which was obvious. In the latter stages of this affection,

the right side of the heart, the auricle especially, always becomes much thinned. Its wall is reduced, by the separation of the muscular fibres consequent on the habitual distension of its cavity, to an expansion of its inner and outer coverings; and in such cases there will always be found to have been deposited a laminated soft exudation over one inch or two of the external surface of the auricle corresponding to the thinned portions.

b, With thickening of serous lining of left auricle.

From some observations which I have made on another division of this subject, I have had reason to infer that the pulmonary artery is more frequently diseased in its coats than is usually believed; and that, when such a condition of this vessel exists, its vestiges are observable in the right ventricle, and also often in the left auricle. Of the two following examples of well marked simple congestive disease of the heart, there was in one, this complication of thickening of the auricular endocardium on the left side, and in the other, the same state existing in the mitral valve, but the condition of the pulmonary artery in the lungs was not noted.

Case 21.—*Bronchitis of considerable standing, with continuous bellows murmur in heart, treated for two weeks by squill and calomel. Sudden debility of heart, relieved by hot bath and stimulants. Begun improvement in another week, and in two weeks cardiac murmur gone. After a few days, an attack of pleurisy followed by diarrhœa, and in two weeks and a half, by rigors, unasarca, and return of bellows sound in heart, afterwards by partial paraplegia and hæmoptysis, and at the end of a month more by death. Usual changes in heart, and thickening of auricular lining on left side.*

Mary Dewar, aged 36, married, with children. This woman was admitted on the 14th January 1848. After exposure to the weather, she was seized eight months ago with œdema of lower limbs and belly, which continue, with some fluctuation in latter. Pulse 114, feeble; tongue scarlet coloured; cardiac dulness extensive, and sounds single and accompanied by a continuous blowing murmur. Subcrepitous rattles pretty general in lungs. Habitual dyspnœa; epigastrium tender. The liver was felt enlarged.

To take half a pill of calomel and opium every third hour; one dose of the diuretic powder without foxglove every sixth hour; to use imperial for drink; and to have the feet placed in the hot sinapised bath for one hour.

27th, Radial pulse gone; pulse at humerus 124. Some pulsation in jugular. A continuous rasping sound heard over whole region of heart and in neck. Is loudest over aortic valves, and least so over right ventricle; vertigo, and indistinct, or double vision.

To be placed in the hot bath; to have two ounces of gin daily; to take, every second hour, half an ounce of the mixture of carbonate of ammonia. Continue diuretic powder. Omit calomel and opium pill.

Feb. 1, Cardiac murmur heard as a moderately strong rasping sound only under left mamma; in other situations there is neither rasping nor blowing, but simply a prolonged continuous natural sound. Dyspnœa much relieved; vision improved; pulse 96. The patient lies on either side. Medicines to be continued.

2d, Much superficial tenderness in right infra-mammary and hypochondriac regions.

Gin to be stopped, and a sinapism to be applied to the pained part of the breast.

7th, Cardiac sounds are now simply a little harsh, but are quite without murmur. The patient is dressed, walks about ward, and can lie in any position.

To take three times daily two ounces of infusion of quassia. Continue mixture of carbonate of ammonia.

9th, Much pain of right thorax; pleuritic friction sound under left axilla. Diarrhœa. Pulse 108, full.

To lose by cupping from the pained part of the chest six ounces of blood; and to have applied afterwards to the right side of chest warm fomentations. To take the chalk and catechu mixture according to the symptoms. Milk diet.

Twelve grains of calomel and four grains of opium, with a sufficient proportion of electuary of catechu, to be formed into twelve pills; one of which is to be taken exery sixth hour.—Other medicines to be omitted.

After the application of a blister to the side, a marked return to a kind of convalescence again occurred; infusion of colomba was used at this stage with benefit. But on the 1st March a return of the anasarca, continuous cardiac plane sound, and rigors, were repeated. The symptoms were treated again by a mild mercurial course, blisters over heart, diuretic powders, and some gin; and on the 18th, a quarter of a grain of nux vomica was ordered, with some squill and cream of tartar every four hours.

21st, Ascites and anasarca increasing, with loss of power in lower limbs and greater feebleness of the general circulation.

29th, Hæmoptysis. Lies indifferently on either side, but not on back. Pulse gone at wrist; short, rapid, plane sound in heart. She died on the 3d April.

Inspection.—The right auricle of heart was dilated to more than twice its natural size. The circumference of tricuspid opening was six and a-half inches. The right ventricle was enlarged, and its thickness at apex was $\frac{2}{8}$, and at root of pulmonary artery about $\frac{1}{8}$ of an inch. The left auricle was dilated to about one-half more than its natural size. Its opening into the ventricle was about one inch in diameter; the lining membrane of this auricle was of a light buff colour, thickened, and easily detached in the form of a tough fibrous membrane. The left ventricle seemed capable of containing an ordinary-sized orange, and the thickness of its walls varied from half an inch to one inch. The calibre of the aorta at root was two inches and three-quarters, and of pulmonary artery nearly five inches. The heart weighed fifteen ounces and a half. The valves were healthy.

Case 22.—*Probable chronic bronchitis, pregnancy, albuminuria, acute bronchial attack, after parturition; anasarca and ascites. Double cardiac murmur, jugular pulse, orthopnœa and death on the 7th day from admission. Usual changes in heart, and thickening of the mitral valves.*

Mrs Macfarlane, aged 35, admitted on the 5th October 1847. Three months since, this woman, after delivery of a still-born child and exposure to cold, was seized with the symptoms of bronchitis and anasarca. Chief complaint at present is of the distension of the belly. She is exhausted, is inclined to retch, and has diarrhœa. The appetite is said to be good. The pulse is 92, and hard. The tongue moist, chopped, and of a pink colour. The jugular veins are distended and pulsating. A double cardiac murmur, which is best heard over lower third of left ventricle, and some cardiac dulness. Double breathing, with sonorous rattles general over thorax. Urine sp. gr. 1.817, and albuminous.

Sudden orthopnœa, and death on 12th.

Inspection.—Escape of serum from abraded cuticle and natural openings.

The abdomen was meteoric, and contained about four gallons of serum. The right lung was œdematous and weighed twenty-four ounces, the left fourteen ounces. The heart was distended with clotted blood, and enlarged. The diameter of the right auriculo-ventricular opening measured two inches, and the chords of the valve were loaded with fibrine. The circumference of the pulmonary artery measured three inches; that of the root of the aorta two inches and a half, and communicated a sense of constriction to the index finger throughout the first inch of its length. The left ventricle might hold about six ounces of fluid. The mitral opening was about half an inch in diameter, and its valves contained several thickened points. Other valves healthy.

The liver weighed seventy-two ounces. The *lobuli* were enlarged, and consisted each of a deep red coloured central, and a paler external portion. The capsule was much thickened, and the structure of gland was softened.

The right kidney weighed six ounces, and the left nine. Structure of a light yellowish gray colour, sprinkled by lines and points of a purple colour.

In the case with which I conclude this subdivision, the complication obtaining in the left heart was so great, that I gave the diagnosis on admission, as one of pure sinistral or valvular disease. It is matter of regret to me, that, in consequence of the condition of the pulmonary artery throughout the lungs, and of the aorta throughout the trunk, not having been investigated, as well as from the want of a detailed early history, there is still some doubt in regard to the precise pathology of the case. With these exceptions, the account of the inspection is so specific, that I am unwilling to lose it, and the circumstances throughout so favour the exposition of them which would place the case under the present head, that I am induced to give it.

Case 23.—*Two years before admission, and after a cold, had anasarca, palpitation, and dysury. Eighteen months afterwards, had a similar, and a fortnight before admission had a third, attack, with cough, expectoration, and dyspnœa. Continuous bellows murmur in heart. Various changes, and death in six weeks.*

James Howatt, aged 18, a boiler-maker, was admitted on the 18th of May 1848. About two years ago, after exposure to cold, his feet and legs began to swell, and he was affected with beating at heart, indigestion, and frequent painful micturition. He received much benefit from treatment; but six months ago had a return of the same symptoms, which again yielded to same means, till a fortnight ago, after a renewed chilling, the symptoms reappeared, accompanied by slight cough, expectoration, and severe fits of dyspnœa, especially during night, causing him to start up in a great flurry. These symptoms are much increased by turning to left side. The urine is scanty, sp. gr. .1026, and slightly albuminous. The tongue is clean; pulse 96. The person is slightly œdematous all over; the lips are livid; the thorax, especially on the left side, bulges forwards. The heart's impulse extends from about one inch to the left of left *mammilla* to the right side of sternum, and from fourth rib superiorly to within an inch of umbilicus. Dulness stretches about two inches across and four vertically. Murmur, which is a continuous blowing sound, is heard with greatest intensity from the site of the aortic valves downwards and outwards around mamma. It is also well heard over whole of right ventricle, the ascending aorta, and in the neck. The carotids throb strongly; but there is no throbbing in any other artery, and the jugular veins are not enlarged. Respiration is bronchial. Constriction of abdominal parietes immediately above umbilicus as if from habitual use in breathing. Cough consists generally of a single, dry, and rather husky, respiratory effort. The chief complaint is of the dyspnœa, and occasionally of the palpitation.

Down to the 25th June,—that is, about six weeks after admission,—the urine never exceeded two and a half pounds daily, and the symptoms acquired a gradual increase in general severity. The diuretics employed were the usual small doses of the calomel and opium pill, the diuretic powders of squill, cream of tartar, and nitre, some imperial, and two ounces of gin daily. On the 25th June the urine rose to three pounds, on the 26th to twelve pounds, and on the 27th to sixteen pounds; and on this latter day the report is,—

Anasarca and dyspnœa much diminished; pulse 96. Bellows murmur entirely gone except in ascending aorta and pulmonary artery, in both which there is a pretty loud diastolic blowing sound. This is heard slightly only at aortic valves; at the apex the sounds are loud and ringing, but without murmur, and at the axilla they are single, and also without murmur.

Remedies to be continued.

Immediately after this report he was attacked with pain in the right hypochondrium; the inspirations rose to thirty-two per minute; the urine fell to one pound and a quarter daily; and he now preferred to lie on the left side. No influence was exerted on the kidneys by any combination of diuretics; and he died on the 9th July.

Inspection forty-eight hours after death.

Precordial dulness extends seven inches vertically, and five laterally. The circumference of the left thorax was sixteen inches, that of the right sixteen and a half. Œdema slight, and present only on fore-arms and legs. The fat under skin was absorbed, and the muscles of thorax were attenuated. On removing the anterior part of bony thorax, at cartilages, the heart was seen to stretch across nearly the whole cavity, a small portion of the edge of the left lung only being visible. The heart in pericardium measured, directly across at fourth rib, six and a half inches; obliquely, from right auricle to apex, it was seven; and at base of ventricles it measured six inches. The pericardium was vascular, thickened, and contained three ounces of serum, but was otherwise healthy. The coronary vein over right ventricle of the heart was as large as a goose's quill. The vertical diameter of the right auricle was five inches, the lateral diameter two and three-quarter inches. The vertical length of the right ventricle was six inches; transverse diameter of the same three inches and three quarters. The left ventricle was five inches and a half in length, three inches and a quarter of antero-posterior diameter, and two and a half across. The circumference of the whole organ was thirteen inches and a quarter. Its weight, with roots of vessels attached, was twenty-seven ounces and a half.

The right auricle might be competent to hold a small orange. It was filled with a firm fibrinous clot, and its walls were dense and persistent when emptied, in consequence of its outer surface being covered by various arborescent bands of firm, opaque, and organized lymph. The right ventricle was not thickened in its substance, but was covered over by fat. The right auriculo-ventricular opening was permanently open for about the diameter of an inch.

The pulmonary artery, slightly stretched, gave one inch and a-quarter across, and its external circumference at root, when not distended, was three and a-quarter inches.

The tricuspid valve was thickened and whitish in colour in several parts, and the root of the pulmonary artery was more opaque than common.

The left ventricle was dilated into a large flaccid bag. The mitral orifice was slightly thickened, occupied by a few minute warty granules, and measured three-fourths of an inch in its long, and nearly half an inch in its short, diameter. The aortic orifice was externally about two and two-eighth inches in circumference, its valves slightly thickened, and sprinkled by a few very small sized warts. The endocardium of the ventricle was white and thickened; its walls five-eighths of an inch in thickness. The septum was half an inch, and, in some places, three-quarters of an inch thick. The lining of the left auricle was a little thickened. The external circumference of aorta at root was two inches and a-half.

The right lung weighed nineteen ounces, and its pleural cavity contained about thirty ounces of fluid. There were some adhesions on it posteriorly, and between it and the diaphragmatic pleural surface, there was an abscess which contained about one ounce of ill-digested and bloody purulent matter. The lung itself was smaller than natural, and collapsed on opening the chest. A few hepatized portions were dispersed throughout its upper lobe, and its bronchial membrane was thickened and congested. The left lung weighed twenty-one ounces. It was much congested, and contained more solidified portions than the right. It was attached to the diaphragm and ribs by numerous old adhesions, but was, nevertheless, pressed up by the increased bulk of the heart, so that the stomach rose nearly to the level of the left nipple. Its bronchial lining was also much thickened and congested, and its cells were distended by frothy serum.

The liver weighed five pounds, was granular, firm, and extended nearly to umbilicus inferiorly; while above, its upper surface pressed on the right ventricle of the heart.

The left kidney weighed eleven ounces, the proper renal tissue being interlaid with a fatty looking deposit. The ureters were slightly congested.

c, With deposition of atheromatous or calcareous matter under the serous coat of aorta.

When sanguineous congestion of the kidneys exists permanently, from whatever cause, whether as an acute condition dependent on the application of cold to the surface, either in a state of previous health, or of convalescence from febrile or exanthematous disease, or as a chronic condition such as is produced by pregnancy, or impeded circulation through the lungs or heart,—the alterations produced in the constitution of the general mass of blood by the functional derangements of the kidneys, soon give rise to inflammation, with its various changes in the serous and fibrous tissues of the heart. These, in cases of primary albuminuria, arise in the heart as secondary affections, and give occasion to more or less thickening and opacity of the endocardium, attenuation, dilatation, and flaccidity of the cavities, and also, as I suppose, in other cases to thickening and ossification of the fibrous tissue of the heart at its openings. It has seemed to me that a most frequent result of this albuminous cachexia on the heart is also the generation of warty and fungous excrescences around its outlets. It would appear that, just as in constitutional syphilis, a low kind of inflammation is induced wherever there is much attrition, as in the throat, nostrils, angles of the mouth, and anus,—so, in the present instance, the diseased constitution of the tissues and of the blood occasions somewhat similar effects. Such warty growths are occasionally to be met with as complications also to simple congestive disease of the heart of long standing; and when they do so, they must be regarded as a kind of tertiary products, the result of diseased blood originating in the destruction of the kidneys, itself a secondary effect of the previous affection of the heart.

Another frequent pathological change is, as is well known, the deposition between the serous and fibrous coats of the pulmonary artery and aorta, as of the arteries generally, of a material which, in its physical characters, chemical composition, and mode of deposition, greatly resembles the matter of tubercle,—the atheroma of authors. I cannot at present speak of the causes of this deposition from personal knowledge; but would treat of it here only as an occasional complication of simple congestion of the heart, of which the following are examples.

Case 24.—*Emphysematous lungs; acute bronchitis, ending in pneumonia and death; dilatation of heart, and some atheroma of aorta.*

James Murdoch, aged 21, a weaver, February 9, 1847. Eight days ago this man had rigors, followed by increased heat, headach, pain in back and anterior lower thorax, with severe cough, for which he was bled, without relief. The latter symptoms are now more aggravated. The face is sublivid: the expression anxious. Inspirations 48; pulse 128; sinistral decubitus; sputum very copious and purulent; tongue violet coloured and coated yellow; much thirst; total anorexia; bowels loose; chest dilated, precordial dulness gone; second cardiac sound preponderates much; respiratory murmur supplanted everywhere by loud subcrepitant or submucous rattle.

To take every second hour a pill of calomel and opium. To have every second hour half an ounce of a mixture consisting of one grain and a half of tartrate of antimony, in eight ounces of mucilage. To have the chalk and catechu mixture if requisite. A sinapism to be applied over the fore part of the chest.

10*th*, Extensive mucous wheeze in thorax.

To have immediately, as an emetic, half a drachm of powder of ipecacuanha. Sinapisms to be applied in succession over the whole chest. To have every second hour the third part of one pill of calomel and opium.

12*th*, Diarrhœa; much exhaustion; pulse 108; pain of back.

Medicines to be omitted. To take every second hour half an ounce of the mixture of carbonate of ammonia. A little food every third hour; and if diarrhœa continue, let an enema containing 25 minims of laudanum be administered.

13*th*, Dyspnœa, and extended moist crepitus.

To repeat the ipecacuanha powder as an emetic.

15*th*, Pulse 120, full; respiratory symptoms as at last report, with some dulness of left posterior thorax.

To take the third part of one pill of calomel and opium every second hour.

16*th*, Nearly moribund.

To omit pills. An enema, containing two ounces of spirit, to be administered immediately. One ounce of wine every third hour.

17*th*, Pulse 120, feeble. Increased dulness on percussion over whole of posterior aspect of thorax, with moist rhonchi of different kinds throughout lungs. Surface cold; face sunk; inspirations 40; slight delirium.

To repeat the enema of spirit. Continue wine.

18*th*, Death at 4 P. M.

Inspection.—Lungs did not collapse; they completely concealed the heart, and their surface was variegated by many opaque white patches and bands. The posterior and outer aspect of each was in a state of red hepatization, impermeable to air, and yielding on pressure a highly bloody serum. The bronchial tubes contained no purulent matter; but their lining membrane was considerably thickened. The bronchial glands were enlarged, firm, and black.

The left ventricle of the heart was dilated with much hypertrophy; the other cavities were enlarged, though in less degree. The whole organ weighed fourteen ounces. The structure of the valves was unaltered. A few minute white elevations were seen on inner surface of ascending aorta; pericardium natural.

The liver of great size and weight, from sanguineous congestion; bile viscid and dark. The kidneys were granular.

Case 25.—*Chronic asthma of four years' standing. Lividity of face, jugular pulse, dyspnœa, anasarca, and, ten days after admission, a cardiac murmur. Relief of the dropsy, and death in a month from diarrhœa. Great dilatation of heart, with atheromatous patches in aorta.*

Henry Dewar, aged 36, a quill-dresser, admitted May 18, 1847. Four years ago this man had a severe fit of asthma after exposure to cold, and since that period he has always suffered more or less in his breathing. Cough has gradually become more troublesome, and, about seven weeks ago, compelled him to give up work. At present he lies on the left side; he is anasarcous, and the belly is fluctuant. Lobes of ears, lips, and gums are of a purple hue. Respiration 36 per minute, and laborious. The cough is paroxysmal, sputum viscid; venous pulse in neck; increased action of carotids. Cardiac dulness extended; impulse weak; sounds appear natural. Pulse 90, weak; tongue furred, moist, and tremulous. Bowels regular; sleeps soundly; has been intemperate.

He was put on fractional doses of calomel and opium, the powder of squill, with cream of tartar, and two ounces of gin daily; and he had, in consequence of diarrhœa, the catechu mixture.

30th, Face, but especially nose and ears, become nearly livid when patient is asleep. A systolic murmur is heard over the situation of the tricuspid opening, and extending to the right auricle above the corresponding nipple. A continuous murmur is heard in the situation of the aortic valves. Urine five pounds. Is much disposed to dose, and has diarrhœa.

To take every fourth hour the diuretic powder; and every third hour the third part of one calomel and opium pill.

June 12th, Mouth sore. Lividity of face, and dyspnœa lessened. Urine five pounds.

To have calomel and opium pills every second day only; continue the powders.

23d, Anasarca has disappeared much, and the respiration continues to be relieved. Dysenteric diarrhœa.

One scruple of sugar of lead and three grains of opium to be mingled and formed into six parts, one of which is to be taken every sixth hour.

24th, Death. Its particular circumstances not recorded, and not now remembered.

Inspection.—Universal adhesion of lungs to walls of chest of long standing. The bronchia were dilated, and the lungs emphysematous. The pericardium contained ten ounces of straw-coloured serum. There was much dilatation of right heart, with concentric hypertrophy, and some diminution in size of left ventricle Some lymph lay exuded under proper covering of right side of heart, and there was great enlargement of the coronary vessels. The right auriculo-ventricular foramen gave passage to four large fingers; the pulmonary artery to two, and the aortic and mitral openings to one only. The valves were healthy; but the aorta was studded and thickened by atheromatous depositions.

Much fluid was contained in the abdomen. The liver was in a state of fatty degeneration. The spleen was large, and contained tubercles. The kidneys were in the second stage of Bright's disease.

d, Complication with hæmoptysis.

The ordinary mode of termination of simple congestive disease of the heart, when it proves fatal, is by serous effusion into the pulmonary tissue, the pericardial and pleural cavities. Occasionally the closing scene, however, is, as has been noted in several of the preceding cases, marked by the effusion of blood; and sometimes the course of the affection is prematurely arrested by fatal hæmoptysis; or there is an intercurrent occurrence of pulmonary hemorrhage, from which, under suitable treatment, the patient recovers. The diuretic powders prescribed in the following case contained each half a grain of digitalis, a grain of squill, and five grains of nitrate of potash, with a similar quantity of cream of tartar. Their use, as well as that of the pills of calomel and opium, was continued for fourteen days. There may be room for difference of opinion in regard to the quantity of wine prescribed. There is none in respect to the principle of its exhibition. The case, like all those not occurring under my own care, is copied from the journals of the house by a competent person, without my knowing anything of the physician who treated it. In the present instance I cannot withhold my approval of the general principles on which that treatment proceeds.

Case 26.—Eliza Colquhoun, aged 30, December 1, a millworker. For several years this woman has been labouring under cough and dyspnœa, and a month ago feet and legs began to swell. The abdomen is fluctuant; right hypochondrium tender; percussion of chest clear; respiration in left side bronchial; in right dorsal region there is loud mucous crepitation, and anteriorly some rubbing sound. Urine scanty, but without albumen; menses absent for five years. Face livid and puffy; pulse

112; tongue of a livid white colour, and clammy; sputum a thick, slimy, dirty coloured mucus. No abnormal sound accompanies the action of the heart.

To have immediately half an ounce of castor oil. To take three times daily one pill of calomel and opium; and three diuretic powders daily. A blister to be applied to the lower part of the breast.

Dec. 3d, Breathes more easily; pulse 120, soft; rather small.

To have daily three ounces of malt whisky. Continue other remedies.

6th, Bowels open; pulse 100, soft; dyspnœa and lividity of lips; large crepitation at right mamma, and numerous sonorous rattles throughout lungs; percussion clear; urine one pound and a half.

To give up spirits; and to have six ounces of white wine daily. A blister to be applied near the breast. Pills and powder to be continued.

12th, Dyspnœa much increased, and livor of face more intense. Pulse very indistinct at wrist; urine two pounds; sputum a yellowish opaque mucus; bowels open.

Wine to be increased to eight ounces daily. Continue other remedies.

13th, Pulse 100, soft, very small; tongue livid, white, and dry; dyspnœa rather less; anasarca as before; urine two pounds; physical signs as formerly; some ptyalism.

Wine to be increased to twelve ounces daily.

Continue other remedies.

15th, Omit pills and powders.

Wine to be increased to nineteen ounces daily.

16th, Hemorrhage from lungs, and death at 7 A.M. on 17th.

Inspection thirty hours after death.

Chest.—The left lung was adhering to costal pleura, with slight effusion behind. This lung generally was much congested with blood, and its bronchi were of an intense red colour, and filled with a bloody serous fluid. The right lung was more healthy, but had some old adhesions. The heart was large, but natural in internal structure. Four ounces of opaque fluid were contained within pericardium.

The liver was natural in size, but covered by an organised false membrane. The kidneys and spleen congested, but otherwise healthy.

In a second example of this lesion, there may be a question with some as to the source of the blood; it seems to me to have been the lungs.

Case 27.—Thomas Maclean, aged 42, a labourer, 28th October. This man has been short of breath for several years, but continued his employment till eight days ago, when he caught cold. His breathing is hurried, difficult, and full of wheeze; cough occurs in frequent, severe, and protracted fits; expectoration scanty and frothy; pulse 72, regular and full; thirst very great; appetite bad; severe headach, with cough; cardiac sounds heard with difficulty, and impulse gone; loud sonorous and wheezing, with other bronchitic sounds heard over lungs.

To take immediately one scruple of ipecacuanha, with one grain and a half of tartrate of antimony; and to have a blister applied over the breast.

30*th*, To take three times daily a pill, containing three grains of blue pill mass, one fourth of a grain of tartrate of antimony, and half a grain of opium; and to use squill mixture.

November 2*d*, During night was seized with severe dyspnœa, and an acute stitch at the lower part of the right side, when four ounces of blood were taken from arm with relief. Dyspnœa continues severe, with much noise apparently from an obstruction at upper part of trachea; pulse 120; tongue brown and dry; thirst urgent.

Blood to be drawn from the right side by cupping; and to take every hour half a drachm of the antimonial solution.

3*d*, Pulse 116. Has had vomiting, and several loose stools; dyspnœa and wheeze as yesterday. There is a superficial ulcer on left side of uvula. Pain of side, which was relieved, has again returned. To repeat the cupping from the right hypochondriac region; and to have every third hour a pill, consisting of two grains of calomel, one fourth of a grain of tartar emetic, and half a grain of opium.

11*th*, The mouth is very sore. Breathing had become easy on the 5th, and the powders were omitted on the 8th.

13*th*, Died after vomiting about three pounds of florid blood.

14*th*, *Inspection*.—Mucous lining of larynx thick and hardened, but of natural colour. There was a clot of blood resting on the epiglottis, and another in the *rima glottidis* and upper part of the larynx. The *bronchi* in many places had similar dark and loose coagula, and the calibre generally of these tubes was increased. The parenchyma of the lungs was infiltrated in numerous places with blood, and such portions, when sliced, had a granular appearance from the blood so effused. Both lungs were larger than common, and the parts which remained free from sanguineous engorgement were emphysematous. The pulmonary structure, which was impervious to air, amounted to two-thirds of the whole.

The heart was large, and the cavities filled with fibrinous clots.

The liver was hypertrophied, congested, and slightly granular in its structure.

e, Complication with sanguineous congestion of the lungs without hæmoptysis.

This occurs under two general forms. In one, the pathological condition is simply an exaggerated form of that state of pulmonary engorgement which is the essential element of dextral cardiac disease, and is sometimes the precursor or accompaniment of hæmoptysis. In such circumstances, the seat of the congestion is usually the posterior or postero-inferior parts of the lungs, and very generally of the right lung. In another form, the congestion is chiefly situated in the apices of the lungs. This variety is often preceded by albuminuria, and succeeded by tubercle, or conjoined with it.

Case 28.—Mary Green, aged 40, married, admitted on 19th April 1848. This woman has been troubled for several years with cough, attended at times by expectoration. About four months ago, the feet, legs, and belly became œdematous, which state continues at present. The face also is slightly swollen, and the cheeks are livid. She complains of much cough, dyspnœa, and expectoration of viscid mucus. Dulness on percussion in both clavicular regions; crepitant and mucous rales general. At apex of right lung the breathing is cavernous, and there is some gurgling, and great resonance of voice. The jugular veins are much enlarged and slightly pulsating. A continuous plane sound, which is best developed at its hinder or diastolic portion, and best heard around the outer aspect of the left mamma and in the epigastrium, but not at all in the neck. The liver is felt indurated and projecting. Urine very scanty, and of a deep red colour, depositing a sediment on cooling. Bowels regular; tongue pretty clean. Pulse 80, soft and weak.

To have two ounces of gin. To have every fourth hour half a pill of calomel and opium; and every sixth hour the diuretic powder without foxglove. The feet to be often immersed in hot water.

22d, Death. Inspection on 24th. Collapse of lungs on opening thorax. Each pleural cavity contained about four ounces of clear serum. The apex of the right lung was very firmly attached by old adhesions to the thoracic walls. Apices of both lungs indurated for about three inches down, but without tubercular deposit. The inferior parts of lungs much congested, the bronchi filled with viscid mucus, and mucous membrane injected. The right lung weighed seventeen ounces, and the left sixteen ounces.

The heart and coronary vessels were much enlarged; the pericardium contained about two ounces of clear serum, and the heart was twelve ounces in weight. The walls were about their natural size, and the valves were enlarged in structure; but the capacity of the cavities and openings was increased.

About two pounds of clear reddish serum were contained within the peritoneum. Stomach and bowels much distended.

The liver was of small size, and indurated. It weighed only one pound and three quarters. The spleen natural. The kidneys weighed each about seven ounces; they were in the first stage of granular degeneration.

f, Complication with tubercles in lungs.

In *phthisis pulmonalis* the cavities of the heart, as is well known, are, for the most part, smaller than natural; but when superinduced on already existing dextral or simple congestive disease of the heart, the distinguishing pathological appearances, and also many of the symptoms, of the latter state, are found blended with those peculiar to the tuberculosis.

Case 29.—William Graham, aged 50, a cabinet-maker, 7th February 1829. This man has had cough and dyspnœa for the last two years, and pain and œdema of legs for the past ten days, all which symptoms continue, with some lividity of face, tumidity of abdomen, frothy muco-purulent expectoration, and epigastric pain shooting into cardiac region, and aggravated at night. Is emaciated; urine scanty; micturition difficult; thirst urgent; occasional uneasiness at stomach as of hunger; pulse varies from 96 to 130, and patient sometimes sweats profusely. Death (on 17th) in ten days.

Inspection.—Lungs red, generally inflamed, their whole surface covered with lymph, and adherent to inflamed costal pleura. Both pleuræ contained serum, having flakes of lymph floating in it. Both lungs emphysematous, but their general structures impacted with incipient tubercles. The tubes were choked with frothy muco-purulent fluid, and their inner surface was much inflamed and thickened. The heart was much enlarged, and the pericardium contained about three ounces of serum.

Case 30.—Isabella Winter, aged 32; stay-maker; single; admitted on April 6, 1847. This woman has had cough, with expectoration, for eighteen months. Symptoms commenced after exposure to cold. Throughout the late winter has also had much dyspnœa. All the symptoms have, however, been more distressing than ever for four weeks past. Has fallen off much in flesh and strength. On admission, dyspnœa is very urgent; face sublivid. She complains mostly of pain and tightness at throat, and there is loud tracheal wheeze. Percussion of chest is fair. Over every part of both lungs, there are heard moist rales of various degrees of coarseness, accompanied, in various situations, by sibilus. At either apex, there is breathing of a bronchial, or almost cavernous character, with very loud bronchophony, and a loud creaking respiratory sound. Sputum very copious, consisting of large adhesive purulent masses, with a few bloody streaks. She complains also of cardiac palpitation; heart's action violent; sounds appear fair, but cannot be satisfactorily heard, on account of the loud pulmonic noises. Prefers decubitus on either side to that on back. Sweats at night. Slight œdema of feet. Pulse 104; appetite irregular, sometimes preternaturally great; tongue slightly foul; much thirst; bowels loose for two or three weeks; menses absent for five or six months.

She had a mucilaginous mixture for the cough, and three ounces of gin daily, but died on the morning of the 3d (ninth) day.

Inspection.—*Thorax.*—Both pleuræ were adhering at almost every point by firm fibrous attachments. The left lung presents several small masses of yellow tubercle, of firm consistence, in its upper and middle lobes. The bronchial membrane is reddened, and the tubes contain much muco-purulent fluid. The tubes generally are of greater calibre than usual; and in the upper lobe, close to the apex, there are two dilated into pouches of fully one half inch in diameter.

The right lung contained a very few yellow firm tubercles near apex. The bronchial lining presents the same redness, and tubes the same contents and general dilatation as those of left lung. In both lungs, there are many small grey tubercles, and, in both, there is much engorgement, with loss of elasticity in depending portions.

The heart weighed nine ounces and a quarter. Its structure was healthy, but its right auriculo-ventricular orifice transmitted four, and its left three fingers.

Abdomen.—The liver weighed forty-eight ounces. It contained much blood, and the colours on its section were very vividly contrasted. The spleen weighed seven ounces and a-half; it was soft, and almost black. The kidneys were natural; that of right side weighed five ounces; that of left, five and a-half.

The *alimentary canal* presented, almost throughout, evidences of venous turgescence. This was most marked in lowest two or three feet of *ileum* and upper half of colon, where the lining had an almost uniform dark red colour. Many of the solitary glands, at the lowest part of the *ileum*, were unusually prominent. There was no ulceration, nor could any trace of Peyer's patches be detected. One mesenteric gland was converted into a chalky mass of about the size of a common marble.

g, Complication with cerebral disease.

In individuals affected with dextral cardiac congestion and predisposed by physical conformation to cerebral congestion, or whose occupations tend at once to impede the pulmonary, and perhaps to exert some influence on the abdominal circulation, as in basket-makers, tailors, or miners who work in a thin seam in a constrained posture,—the brain sometimes suffers from the engorgement of the right heart more remarkably than any other organ. The following are some examples.

Case 31.—*Apoplectic constitution; jugular pulse; precordial sonorousness; prolonged diastole; sudden access of vertigo; headach and œdema of legs.*

John Walker, aged 57, a weaver, April 4, 1840. Is of a robust make, florid complexion, and short neck. Four weeks ago he was suddenly affected with severe giddiness, loss of sight and memory. Three weeks ago he was seized with dull, deep-seated pain in temples and forehead, which has, as well as an inability to walk, turn his head, or look steadfastly at an object without great vertigo, increased down to admission.

There is a deep hollow at the ensiform cartilage, and the precordial dulness is gone in ordinary states of the breathing. Jugular pulse; cardiac impulse not perceived, even on a deep expiration; heart sounds acute, and the diastolic is long and grating. The liver is not sensibly enlarged. Two weeks ago he had slight cough, with some expectoration of white, tough mucus, followed by slight swelling of lower limbs. Abdomen tumid; pulse 76; bowels regular.

This man's mouth was made sore by a pill, consisting of blue powder, ipecacuan, and extract of chamomile; he had the black draught every second night, prolonged hot foot-baths every night, the diuretic powder every six hours; and after three weeks, was put on half-drachm doses, twice daily, of the saccharated carbonate of iron, with infusion of colomba, and was dismissed relieved four weeks from his admission.

In the following case, a state of albuminuria was interposed between the condition of simple dilatation of the heart and the accession of the head symptoms, and, no doubt, had something to do in the production of these. In the other examples under this subdivision, the cerebral affection seemed to be more exclusively occasioned by the cardiac and pulmonary engorgement.

Case 32.—*Dextral cardiac congestion of some standing; albuminuria; arachnitis.*

James M'Rory, aged 30, a miner, 17th May 1848. Eight weeks ago, after exposure to cold and wet, this man became affected with severe headach, shivering, cough, and white glary expectoration, which continue. He complains chiefly of pain in the head, shooting along back, and has great tenderness on pressure in lower part of lumbar region. Tongue slightly furred; pulse 60.

22d, Lies sunk in bed, reluctant to answer questions, and incoherent in his statements. Pulse 58; some dilatation of pupils, and continuance of headach; spits much; gums healthy, and no expectoration.

23d, Has refused to answer questions since last night, and lies staring at the roof. Pupils irregular and sluggish; pulse 96; passes his urine in bed.

25th, Pulse 120; perspires largely, and continues to stare vacantly; pupils sluggish and dilated; heart sounds strong, and carotids beat visibly; tongue clean; picks the bed-clothes, and grasps at imaginary objects; a large vibex has appeared on sacrum; singultus; neuralgic pain of spine; refusal to speak; and incontinence of stools and urine continue.

26th, Death.

Inspection.—*Head.*—The whole of the venous system of the head was greatly engorged. The brain was much softened, and the ventricles were distended with serum.

Thorax.—The lungs were emphysematous, and very generally occupied by carbonaceous matter, for the most part arranged in clumps of the size of a barleycorn. The mucous lining of whole of the bronchi was much congested, thickened, and of a reticulated appearance.

The heart was dilated to a third or fourth larger size than natural, and its foramina, with the exception of the aortic, which only permitted the passage of one finger, were all capable of receiving two or more. The right ventricle was covered with fat.

The liver was pale; the kidneys were mottled; urine in bladder albuminous.

Case 33.—*Chronic bronchitis, emphysema, and dropsy; relieved. Relapse, in three weeks, and re-admission with same symptoms, and with epilepsy; relieved.*

Margaret Keenan, aged 38, married, mill-worker, was admitted December 15, 1846. This woman had cough and dyspnœa, with œdema of lower extremities, the former of eighteen months' standing but aggravated by three successive attacks of gastro-hepatic fever. The lips were livid; the breathing hurried and oppressed, and there was intolerance of the recumbent posture. The chest was barrel-shaped, dry, sub-crepitant, with sonorous and wheezing rattles heard all over it; percussion clear even over heart, and cardiac sounds exceedingly faint. Frequent palpitation; pulse 108; face œdematous; belly tumid; urine scanty, and free from albumen. The bowels were generally slow; tongue clean; appetite variable; much thirst; headach, and some pain in front of thorax from cough; sputum viscid, serous, and semitransparent; cough worst at night.

To inhale, at intervals, the vapour of boiling water; to have a sinapism applied to the breast; and to take every fourth hour one powder, consisting of half a grain of mercury and chalk, half a grain of ipecacuan, and ten grains of nitre. To take daily half a drachm of compound jalap powder, or half an ounce of oil of turpentine in mucilage, in order to open the bowels. Eight ounces of water to be saturated with the bicarbonate of potass, and three times daily to take one ounce of this solution, a sufficient quantity of nitric acid being previously dropped into the solution, to render it acidulous.

25th, To omit all medicines, and take, every third hour, the third part of one pill of calomel and opium.

29th, Mouth is sore, and symptoms all relieved.

January 2, 1847, Dismissed by desire.

February 12*th*, Says that she continued well for three weeks after dismissal, and that she then began to suffer again from shortness of breath, and some return of dropsy. Symptoms much as before; appetite greater than usual; heart sounds feeble; pulse 96; urine scanty, not albuminous.

One drachm of nitre, two drachms of bitartrate of potass, and six grains of ipecacuan powder, to be divided into twelve parts, one of which to be taken every third hour; and every third hour to take the third part of one pill of calomel and opium.

18*th*, Powders to be stopped; two ounces of sulphate of magnesia, half an ounce of bitartrate of potass, half an ounce of nitrate of potass, half an ounce of ipecacuan wine, and half an ounce of tincture of squill, to be mixed in twenty-four ounces of water, and to take of the mixture two ounces every three hours.

26*th*, Epileptic seizure last night.

To take three grains of sulphate of zinc, three times daily.

This patient is marked as dismissed on 2d March, without any note, but probably, as was my frequent practice when a patient was recovering, and could take any medicine with equal advantage at home, she had some of the sulphate of zinc given her, with directions for its use.

Frequent complications of dextral cardiac disease are met with in the form of acute attacks of hepatic or renal inflammation, and with jaundice, albuminuria, hemorrhoids, and gangrene of the lower limbs and genitals. The jaundice of this disease is commonly occasioned by over-secretion of bile,—a natural effort, as it would appear, of the biliary vessels to relieve the portal load. A similar cause operating on the kidneys seems to occasion the different varieties of diuresis.

h, Complication with diabetes insipidus.

Case 34.—*Cough of nine years' standing after pulmonary inflamation; increase of cough and dyspnœa for seven weeks, accompanied by emphysema, livid tongue, and epigastric pulsation; partial recovery in two weeks; re-admission in fifteen months, having five months previously suffered from influenza and anasarca, and two months before from apoplexy and hemiplegia, all of which gave place to a state of diabetes several weeks before admission.*

Walter More, aged 49, a labourer, admitted January 11, 1847. This man, about nine years ago, had what appears to have been an attack of pneumonia, and almost ever since has been subject to cough, aggravated in cold weather. For the last seven weeks cough has been unusually severe, and has been attended with shortness of breath, with sensation of tightness in thorax; strength has also fallen off. At present slight emphysematous crepitation, and occasional bronchitic rattles are heard over most of chest; chest barrel-shaped, and percussion every where clear. Breathing tranquil, sputum consists of muco-purulent masses, mixed with others which are black and somewhat viscid. Pulse 88, good. Heart's impulse most marked in epigastrium; sounds faint and obscured at base. Bowels rather slow; appetite impaired; does not complain of thirst; tongue moist, large, and dark coloured, but pretty clean; he has never had œdema.

Six grains of ipecacuan, one drachm of nitre, and two drachms of cream of tartar, to be mixed and divided into twelve doses, one of which is to be taken every four hours.

14*th*, To four ounces of the pectoral mixture, one drachm of tincture of squill to be added; and the patient to take one table spoonful every six hours.

16*th*, One pint of London porter daily.

28*th*, Dismissed relieved.

April 21*st* 1848, Re-admitted.

About five months ago, after exposure to wet and cold, he was attacked with influenza, with cough and expectoration, great dyspnœa, and swelling of lower limbs. About two months ago, while rising from bed at night, he was suddenly seized with loss of sensation and motion. Insensibility, attended by hemiplegia of left side, continued for one week, after which the chest symptoms, œdema of limbs, and paralysis, began gradually to improve. About five weeks ago he observed his urine to be increased in quantity, and this now forms his chief complaint; is obliged to rise five or six times during the night to pass water, but cannot say what is the precise quantity he evacuates daily; urine is of a pale straw colour, without sediment or albumen, and is of sp. gr. 1012. Appetite has, always since illness began, been good; bowels regular; tongue pretty clean and moist; respiration slightly oppressed; some cough; little expectoration; anasarca continues absent; chest sounds fair; action of heart regular; second sound preponderates much, and is harsh; pulse 84, full, and pretty firm; some sense of weakness in lumbar region, especially after walking or standing.

This man was cupped to six ounces from the epigastrium; put on infusion of quassia, ammoniated tincture of opium, saccharated carbonate of iron, the tepid bath, and diabetic diet. On the 28th his urine is reported at seven pounds daily; of sp. gr. 1020, and without sugar; and on the 16th May he was dismissed relieved.

The diabetic diet referred to consists of varying quantities of gluten-bread, beef-steaks, and greens; the former being prepared with flour deprived of about fourth-fifths of its farina by elutriation.

I have lost sight of More since his dismissal; but, according to the length his case had previous to that date proceeded, it presents a fair epitome of the disease. The commencement in an acute attack of pulmonary inflammation, the imperfect restoration of the lungs to health, the frequent renewed accessions during an incubating period of nine years, of cough on exposure to cold and wet, and the gradual production of dilatation of the air-cells, accompanied by sense of tightness in the thorax, dyspnœa, and the removal of the heart's impulse to the epigastrium,—constitute a faithful outline view of the incipient and transition stages of the affection; while, on his re-admission, the aggravation of all the above symptoms, the superinduction of anasarca, and then of cerebral disease, relieved for a time by the salutary occurrence of extreme diuresis,—exhibit the malady in its stage of perfect development, or the diseased condition of all the organs. Its termination, not shortened by the accession of a new bronchitic inflammation, would most probably be either in *diabetes mellitus*, or in an arrest of the secretion of urine, and a return of the anasarca,—joint effects of an advanced state of granular degeneration of the kidneys.

It remains that I should offer some observations on the diagnosis of simple congestive disease of the heart, and on the treatment of its advanced stages. Those on the former topic will be more appropriately made in connection with the detail of the general history of sinistral or organic disease, with which it is so liable to be confounded; and with those on the latter I conclude the present division of the inquiry.

The general indications of treatment in the advanced stages of dextral cardiac disease are,—

1*st*, To relieve the more or less extended splanchnic congestion which exists.

2*d*, To augment or sustain the vital or contractile power of the heart.

3*d*, To prevent the recurrence of irregular distribution of the blood.

In reference to the first of the indications of treatment mentioned, or the relief of the venous engorgement of the viscera, the means of fulfilling it vary with the measure and situation of the congestion. In all cases there will be more or less coldness, perhaps wetness of the surface, arterial anæmia of the skin, distension of the superficial veins, lividity of the face, nails, hands, and feet, œdema, arrest of the secretions, and enfeebled action of the left heart. In an extreme condition of this sort, the general warm bath, with or without mustard, has an excellent effect; and by surrounding the body afterwards with bottles or jars of hot water, or bags of heated bran or oatmeal, the effect may be perpetuated. A dose of calomel, followed by another of black draught, or, where there is much loss of vital power in the intestines, by a purgative dose of turpentine, are suitable means of lessening the portal load, and should be resorted to as soon as the animal heat is begun to be restored. Should vascular reaction ensue, the detraction of blood from the epigastric or lumbar regions may be cautiously employed. In great visceral congestion, saline or terebinthinate purgatives are the safest evacuants, and indispensable preparatory remedies to the constitutional use of mercury or the employment of diuretics; but there are individuals in whom such cathartics may be aided by local depletion of blood. Should the treatment of an extreme hepatic engorgement of any standing be rashly commenced by a mercurial course, without the preparation here spoken of, a gastric or an intestinal hemorrhage will most probably ensue; and if we employ stimulating diuretics in a parallel state of congestion in the kidneys, and with a like want of previous means, such as hydragogue purgatives, the warm bath, and perhaps cupping over the loins, for lessening the venous distension of these organs, the sure result will be a marked diminution of the secretion of urine.

The most eligible course, therefore, in the severe cases under consideration, will probably be, after restoring the heat of the surface, and opening the bowels by a turpentine enema, or saline or other purgatives, to take away four or six ounces of blood from some part of the trunk by cupping. Should the icteroid colour of the skin, the disappearance of the impulse or of the first sound of the heart, the indistinctness of the pulse, the coldness of the tongue or breath, the dilatation of the pupils, or the age or other general circumstances of the patient, forbid bleeding, the use of appropriate purgatives and of diaphoretics must be persisted in.

When the liver is the organ on which the congestion in such case is mainly concentrated, the treatment at this stage should be continued with warm fomentations over the whole of that organ. These should be made by wringing out a couple of folds or more of fine flannel from hot water, and covering them over on the parts with an envelope of oiled silk or soft wax cloth, secured by means of another double fold of blanket, plaiden, or thick flannel, and over all by a suitable roller. This application increases the capillary exhalation from the skin in a powerful manner, and often relieves the distension of the liver itself in a very striking way.

Again, placing the patient's lower limbs in a bath which admits of their being immersed to the knees, is an excellent remedy in the same circumstances. Care should be taken that the patches of inflamed skin on the distended limbs do not suffer from the heat, that the patient be not required to maintain a fatiguing posture, and that the full benefit of the bath be secured by having the external air excluded by a light blanket or other covering; and, also, by its use being maintained for as long a period as the strength will allow.

There are frequently occurring instances in which the foot-bath, and much more, the whole warm bath, cannot be used without occasioning syncope. Owing to the burdened heart and circulation which obtain, even slight physical efforts prove hazardous. In such cases the lower extremities and abdomen should be wrapped up in blankets wrung out of hot water, having an outer covering of two or more pairs of dry blankets laid over to prevent evaporation and cooling.

Again, in the same description of jecoral enlargement, the remedies already referred to having been premised, and it being ascertained that no symptom of strongly marked renal congestion is present, benefit has accrued from the free use of the supernitrate of potash, with or without dandelion. One ounce of the saturated solution of the former, and half a drachm of the latter, largely diluted, have been given every six hours. Barley-water, made simply by pouring boiling water on some good barley, seasoned with sugar and fresh rhind of lemon, and running it off when cool, is an excellent vehicle for the supernitrate.

Further, in the same kind of cases, when I did not feel at liberty, owing either to the large bulk or hardness of the liver, to begin the mercury, I have seen very marked advantage from varied combinations of nitrate of potash, bitartrate of potash, hydriodate of potash, sal-ammoniac, sulphur, ipecacuan, senna, rhubarb, and taraxacum or sarsaparilla, in lessening the hepatic congestion. Again, as patients with dextral cardiac disease are almost uniformly met with in winter, it will be essential, if possible, to retain them in an atmosphere not lower than 60°. Flannel coverings should be liberally supplied, and it is often attended with the most pleasant effects when copious sweating can be maintained. Lastly, when no objection to its use exists, or when such has been removed, there is no remedy which empties the liver more effectually, or better aids the diuretic action of squill, than mercury given in frequently repeated fractional doses. When there is reason to fear that its use, even when so given, may stimulate the hepatic capillaries more powerfully than the already engorged or

obstructed larger vessels can make room for, the free and frequent exhibition of seidlitz powders, along with the mercury, will prove highly serviceable by the production of watery stools.

And, under the head of the relief of visceral congestion, the state of the kidneys demands equal circumspection in reference to treatment, as does that of the liver. Patients labouring under a concentrated and acute form of dextral cardiac disease have commonly a very scanty secretion of urine, and frequently there is pain in the region of the kidneys, or along the ureters, and during micturition. Under such a combination of symptoms, there is reason to believe that the general venous plethora has affected the renal organs to a high degree. In chronic cases of concentrated dextral disease of the heart, again, the flow of urine is often found to be greater than natural; sometimes the discharge equals six or eight pounds per diem, and in such the nephralgic pains are trifling. Here the pathological condition of the kidneys is still one of congestion, although of moderate amount.

In the treatment of the first or more acute variety, the immediate use of stimulating diuretics will greatly aggravate every symptom, and, if persisted in, will be followed by troublesome diarrhœa. In the second variety, the same procedure will, within forty-eight hours, arrest the diuresis which exists, and convert the case into one of scanty secretion of urine. In the former event, therefore, it will be requisite to premise one or all of the expedients already referred to for the diminution of the renal plethora, before having recourse to diuretics; in the latter, the case is much better let entirely alone.

In the selection of diuretics, there is not much room for choice. Reflecting on the flaccidity, and, generally, the attenuation of the heart's walls which exist, and on the fact, that a lowered measure of heart power is the centre condition on which all the other morbid states present depend, the inexpediency of employing digitalis is apparent. Its exhibition often relieves the ischuria in a most sudden and effectual way, but its use may be followed by fatal syncope. The variety of the disease in which I was sometimes led to employ it, was that of hypertrophy of the left ventricle, when the heart and carotids were pulsating forcibly; and even in these I conjoined it with various stimulants, as ammonia, nitrous ether, or gin.

Among the stimulating diuretics, I have always been disappointed with the *scoparium*, and have been led to limit my practice to the employment of squill, preceded by a slight mercurial action, and given in combination with one or more of the saline diuretics, aided by copious diluents.

There is a class of cases which resist every effort to produce diuresis. In these, the urine is very often free from albumen as long as they have been under observation, and yet, after death, the kidneys are found indurated, diminished in size, and almost wholly degenerated in their structure.

The second general indication which presents itself in the treatment of the advanced stages of dextral cardiac disease, is to augment or sustain the vital power of the heart.

This is in truth a branch only of the principle involved, in the means already spoken of for the relief of the visceral congestion. A heart distended in all its parts, an arterial system with a diminished amount of blood and of power, and a whole venous system replete and gorged with black blood, are in this disease the essence of what is wrong; and, in seeking the removal of such a condition, our remedies must have reference to the root as well as to the periphery of the disordered vessels.

A singular feature in those confirmed examples of congestive disease of the heart, is the continual vacillation of the symptoms from gradual but progressive improvement to relapse; and, in these successions of change, the fault seems sometimes to lie in the remote capillaries, on one occasion of the liver, and in another, of the kidneys or skin, or, not unfrequently, of the bronchial membrane, while, at other times, the origin of the renewed bad symptoms appears to be in a diminished contractile power of the heart. It is often difficult to appreciate the real import of such changes, and not less so to determine either the time or the degree in which stimulation of the heart should be employed. My most common experience with diffusible stimulants, as gin, ardent spirit, or wine, has been to see their use followed by cerebral symptoms, or, at least, by increased fever and dyspnœa. This has been often true, even in the cases of patients who exhibited what are commonly regarded unquestionable signs of debility, such as the combination of symptoms with which these observations on the treatment commenced. In such patients, the livid colour of the skin, under the action of diffusible stimulants, may be seen to augment with the increasing hardness of pulse and dyspnœa which then ensue, and the treatment will be more happily proceeded in, for a considerable period at least, in the use of the measures mentioned under the first indications. But, in other individuals, probably the subjects of more unmixed attenuation, and softening of the heart, in whom there are present all the more obvious signs of lowered vital power, along with disappearance of jugular pulse when it had formerly been present, and absorption of the cardiac systole and impulse, the cautious employment of ardent spirit or wine is clearly required.

The application of blisters, or of sinapisms, to the region of the heart, will also prove serviceable; and the exhibition of properly regulated doses of the *nux vomica*, or of strychnia, may also be attempted.

The management of the health, in the intervals of freedom from severe symptoms, only remains to be considered. The indications to be pursued with this view are at once to prevent the increase, and also the irregular distribution, of the circulating mass.

The former of these may be attempted, *first*, by placing the individual on dry, although moderately stimulating food, taking care to proportion its quantity to the amount of waste incurred by muscular effort, or cooling through exposure to the weather; *second*, by the employment of tepid or cold sponging of the general surface with vinegar, solution of bay salt, or simple water; *third*, the habitual use of means fitted to increase the natural discharges; one of the most common which I employ for this purpose is a solution in water of Epsom salt, nitre, tincture of squill, wine of ipecacuan, and liquor or extract of taraxacum; and, *fourth*, the continued employment of gentle exercise out of doors.

In reference to the means by which we would seek to maintain an equable distribution of the blood, these are, *first*, the sedulous avoidance of chills from exposure to wet, to cold winds, or to night air; *second*, the protection of the skin by flannel under dresses, proper shoes, &c.; *third*, the employment of tonics, as of the addition to the solution first spoken of, of sulphate of iron or of quinine; and, *fourth*, the successive trial, in varying states of the invalid, of the whole warm bath, or of the hip or foot bath, diaphoretics, ipecacuan, or mustard or chamomile emetics, and of properly conducted exercise.

[Since these papers went to press, I have seen a remarkable example of permanent arrest of the progressive increase of this disease, which had been effected by a protracted residence in India.]

II.—*Of Sinistral, Structural, or Endocardial Disease of the Heart and large Arteries.*

The second general form of cardiac disease to which I make reference in these communications, is that in which the lesions are not primarily and exclusively those of capacity only, and such as are consequent on disease in another system, as in the form already described, but that in which the initial changes are structural, and are limited, also, to the proper arterial system, the hindrance to the circulation being either in the aorta or left heart. In this form the introductory symptoms owe their rise and prominence to the existence and extent of disturbance in the progression of the blood from the left ventricle or auricle; whilst the phenomena proper to the second stage have their source in the multiplied morbid changes which accrue from regurgitation on the left heart itself; then on the pulmonary vessels; and, last of all, on the general venous system.

In proceeding to offer an exposition of both of the above classes of symptoms, I avoid the repetition which must arise from seeking to exhibit the connection of the external signs of each stage, with the respective pathological causes. This formed a prominent part of the plan of the first division of this paper, having reference to simple congestive disease of the heart. In the present division I prefer giving a mere detail of the symptoms of structural or sinistral cardiac disease, extended, without any formal division of stages, throughout their entire course, as they may be observed in cases arranged on the principle of their anatomical character, and, as much as possible, according to what appeared to be the earliest and most important lesion of the parts.

In following out this proposed method, therefore, I shall arrange the cases according as the part mainly affected is the aorta, the aortic valves, the mitral, the aortic and mitral conjoined, the mitral and aortic, or the aortic, mitral, and tricuspid valves; and, as there does appear to be a marked frequency in the association of atheromatous and osseous degeneration of individual parts of the arterial system with tuberculous deposition, I propose to append a few cases, under certain of the above subdivisions, in illustration of this point.

1. Aortic series.

Case 1.—*Aneurism over lumbar vertebrae, with caries of their bodies, and peculiar pain in thigh; death from its rupture.*

—— Lawson, aged 38, a collier. This man has occasional severe, sometimes excruciating, pain in bowels and left groin, the latter extending occasionally to the back part of the thigh, with a sensation as if the thigh were girt with a cord. Muscular strength is greatly deficient; appetite impaired; bowels slow; and he is much distressed, especially after meals, by flatus. These complaints were of fourteen weeks' standing. No tumour could be discovered on examination. He died on the fifth day after admission, while labouring under an aggravation of the preceding symptoms, with faintness, cold perspiration, and hurried breathing.

Inspection.—On opening the abdomen, the peritoneum, on the left side, was seen to be pushed forwards for some inches by a mass of blood. The viscera, which adhered together in many parts, were removed, and the clotted blood being cleaned away, a pretty large swelling was observed on the aorta, about three inches above its bifurcation. On the left side of this tumour, the orifice from which the blood had flowed was discovered, and behind it the bodies of two of the vertebræ were found quite carious, the intervertebral substance between them remaining intact. A quantity of serum was effused into the left pleura.

Case 2.—*Aneurism at upper part of abdominal aorta; death by its rupture; previous symptoms, those of indigestion, excruciating pain in tumour, and profuse epistaxis.*

Margaret Stewart, aged 32, unmarried, a mill-worker. This woman was subject to dyspepsia, broken health, pain in back and palpitation, for two years. During the last eight months these symptoms were much aggravated, and attended by excruciating pain in the belly, for which she has been in the habit of taking two drachms of laudanum daily. Pain general over abdomen, but without tenderness on pressure, except in epigastrium, where a hard pulsating tumour of about the size of an orange is felt. This swelling communicates a thrilling sensation to the fingers of an observer, and, as well as the aorta and heart, emits a loud bellows murmur. Ten days ago she had excessive bleeding from nose, which continued for nearly twenty-four hours, with little cessation, and reduced her almost to death. The menses had been absent for eighteen months. Countenance anxious, and appearance emaciated. Bowels slow, except when she omits the laudanum, when she has some diarrhœa; tongue red and dry, except on sides where it is white; pulse 92, and jerking; death in three days.

Inspection.—Thorax.—The lungs were healthy; the left ventricle of the heart was somewhat hypertrophied, but otherwise sound.

Abdomen.—On opening peritoneal cavity it was seen to be filled with partly fluid and partly clotted blood. An aneurism of the aorta existed at the point of that vessel's emergence from the diaphragm. It had burst anteriorly. The coats of the artery were entirely destroyed, and the sac consisted of several distinct thick layers. The tumour was closely adherent to left kidney, which was atrophied to one half its natural size, while the right kidney was very much enlarged.

Case 3.—*A man in habit of drinking about six glasses of whisky daily for years; palpitation of heart; swelling of superficial abdominal veins; and anasarca of four months' standing; frequent large bleedings from rupture of veins; death; aneurism of abdominal, and dilatation of thoracic, aorta, with enlarged heart, and occluded cava ascendens.*

John Stewart, aged 40, a distiller. This man had some rheumatism about nine years ago, but he is not aware of having had any heart symptoms until four months ago, when he began to be troubled with occasional palpitation. At same time his feet swelled, and the external veins of the trunk became enlarged. The lower limbs were, at the time of admission, greatly distended, as was also the belly, which was fluctuant. Some of the veins were fully half an inch in diameter; heart's action violent, irregular, and accompanied by a loud systolic bellows sound, as low down as the umbilicus; cardiac dulness extends across sternum; pulse about 84; trunk frequently moved by heart's action; a purring sensation communicated to hand laid on abdomen; respiratory murmur loud and harsh over thorax; bowels slow; urine one pound and a half; sp. gr. 1.015; gives no traces of albumen. He was accustomed to take six or eight glasses of whisky daily.

During the following six weeks, nine occasions, on which the distended superficial veins gave way, are reported, with a loss of about eleven pounds and a half of blood. Erysipelas of limbs, and serous oozing from the legs, also occurred, when he died.

Inspection.—Body measures fully six feet, and is greatly swelled, except over thorax, right arm, and face, while the veins can hardly be distinguished. Over the outer aspect of left foot there is an irregular mass of white, granular, honey-like material, of a soft, oily feel, in the situation of the former serous exudation.

Thorax.—Lungs were healthy, large, but not emphysematous. A few old adhesions. The heart weighed twenty-seven ounces. Some old patches of lymph on its surface. The left ventricle was much thickened in its walls, and enlarged in its cavity. The other cavities were much dilated. The valves were healthy. The ascending aorta, arch, descending thoracic, the most of the vessels given off by these, as also the *superior vena cava,* and *vena innominata,* were all greatly enlarged.

On tracing the abdominal aorta downwards to the fourth lumbar vertebra, and about three inches below the cœliac axis, it was observed to dilate into a sac, measuring perpendicularly about three inches, and having a circumference of nine. It was of about the size of the bulb of a Florence flask. The inferior mesenteric artery was given off from its centre, and the common iliacs from its lower part. It contained only a few fibrinous clots. It adhered firmly to the vertebra, and also to the *vena cava,* which latter was much contracted in size at a point corresponding to the centre of the aneurismal sac. Above, the vein was of normal size, but at the contraction it did not admit the tip of the little finger, while below it was greatly dilated, as were also the common iliac veins. The common iliac arteries were also large, but healthy in their coats. The external iliac of the right side was dilated into an aneurism of about the size of a walnut.

The abdomen contained about four pounds of serum, and the cellular tissue under the peritoneum was universally soft and œdematous. Liver pale, friable, and weighed fifty-six ounces and a half. Spleen atrophied and hard; it weighed three ounces. Right kidney soft and flabby, and weighed eleven ounces and a half, while the left weighed only three and a half, and was harder than natural.

Case 4.—*A boy, in whom the aorta appears to have been impervious at the insertion of the* ductus arteriosus *from birth, seized, after exposure to cold at his* 14*th year, with symptoms of pneumonia, which yielded to treatment in eight days, and was suc-*

ceeded in another week by headach, pain in left eye-ball, renewed fever, perspiration of upper parts of body, palpitation, and dyspnœa. Partial recovery in six weeks, and re-admission in five weeks more, with return of dyspnœa, palpitation, and of pain in the left side of the thorax, followed by headach, general oppression, profuse sweatings, throbbing, frequent pulse, cough, disturbed sleep, vomiting, and death at end of second month from re-admission.

Henry Frere, aged 14, a weaver, 3d August 1813. Two weeks ago, after exposure to cold, was affected with dry cough, which, for last eight days, has been attended by pretty copious expectoration and pain, impeding respiration in the left side of the breast. Pulse 100, firm; little appetite; much thirst; tongue white; bowels regular; sleeps ill; sweats a good deal.

This boy, according to another account by the physician who attended him, had a fair complexion and dark brown hair; he was stoutly made, particularly about the chest, and was also well limbed. He had been in Scotland for about five weeks only, but was said to have appeared free from complaint, to have been active, and without any dyspnœa or other apparent uneasiness at his sports.

Blood (which was at first buffy) was twice drawn from the arm, with relief to the pain; and on the 6th the report is, pulse 102, more sharp and strong; expectorates more freely; sputum gross, and, in the other account already referred to, this is said to have been tinged with blood. On the 11th, the report is, symptoms continue to abate, expectoration has less of a purulent hue, and is free of bloody tinge; pulse 79, regular, somewhat hard.

19th, Has complained of headach, heat of skin, and thirst since yesterday. Pulse 104, full. Face and arms perspire.

To have solution of Epsom salts.

The head to be shaved, and washed frequently with cold water.

The tepid bath in the evening.

20th, Is free of headach, but heat of skin and thirst remain, and he complains of pain in the left eye-ball. Pulse 104, less full.

Continue the salts and tepid bath.

27th, Makes no complaint but of palpitation; the first time that symptom is noticed in the journal, " though I rather think," says his physician in the narrative already mentioned, " this was an oversight." He was dismissed, with his complaints much abated, on the 6th October, and readmitted 13th November 1813. Dyspnœa (also omitted to be stated formerly), palpitation at the heart, and pain in the left side of the thorax, returned soon after he left the house, and have been gradually increasing. Pulse 88, regular. Appetite pretty good; considerable thirst.

30th, Palpitation has gradually abated since the application of a blister. The pain also had disappeared completely, but returned last night. Pulse 100, soft and regular.

Blister to be repeated.

December 2d, About three hours ago, was suddenly affected with tremors, heat of skin, perspiration, headach, and general oppression, which continue. Pulse 102, and throbbing.

8th, Complains only of dyspnœa; weakness and pain in left side when he rests on it; cardialgia after meals.

The reports for the succeeding fortnight are occupied chiefly with details of sickness, headach, soreness in the chest, cough, and, on the 23d, of a severe pain in the right side, increased by motion, a full inspiration, dorsal decubitus, and by frequent cough, most troublesome in the night. Pulse 116. He was blistered, used cathartics, and was twice bled, the blood being buffy. The pulse fell to 90, and the pain was removed, but the cough and palpitation continued. On the 27th, the circulation was again quickened, and remained hurried till his death. He sunk in strength; was drenched in perspiration; took no food; was attacked with frequent vomiting; the urine became scanty; his sleep was disturbed; the dyspnœa and palpitation became laborious and violent, and he expired on the 2d of January 1814.

Inspection.—On turning back the sternum, the pericardium presented itself obscuring the left lung, and adhering to the costal pleura. It was thin, transparent, and contained about one ounce of fluid, and a heart nearly twice the natural size. The walls of the left ventricle were remarkably firm, and fully one inch in thickness, but its valves were healthy, with exception of one portion of the mitral, which was larger and stronger than natural. The aorta was expanded near its origin into a kind of pouch, three inches and eight-tenths in external circumference, but, after giving off the arteries to the head and arms, it was as remarkable for its smallness, its external circumference not exceeding four-fifths of an inch, and continued so for about half an inch, or till immediately after its union with the *canalis arteriosus,* where it became very much narrowed, exactly as if it had been tied by a ligature, and was found to be internally completely impervious. The obstruction was about one line in breadth. The artery then all at once acquired an external circumference of 1·6 inches, and received or gave off three trunks of the calibre of crow quills, and also other three smaller, after which it resumed its natural course along the vertebræ, without exhibiting anything remarkable except that about half an inch below the stricture there was a smooth elevation on the inner surface, less raised, but having nearly the diameter of a split pea. These trunks appeared to be the uppermost of the inferior intercostals, but their coats were remarkably thin like those of veins. A probe could be passed along the *ca-*

nalis arteriosus, which was of the size of a crow-quill, from the pulmonary artery to the contracted portion of the aorta; but, from the thickened appearance of the *canalis arteriosus*, as well as from the boy's florid complexion during life, it did not seem probable that any material communication by means of it could be maintained. The left subclavian artery was proportionally much larger than usual, measuring 1·3 inches in external circumference, and it had an unusual curvature to the right. The internal mammary and superior intercostal arteries were uncommonly large, but the epigastric arteries were of ordinary size.

The parietes of the right ventricle were also somewhat thicker than natural, but the capacity of the cavities of the heart seemed normal, and the pulmonary artery was in no way remarkable, except by affording the branch which communicated with the aorta. The lungs were very light coloured, and the left much collapsed. In each pleura there was some bloody serum.

This case is rather a celebrated one. It occurred in the hospital practice of the late Dr Graham, afterwards Professor of Botany in the University of Edinburgh, and an account of it was published by him in 1814 in the 5th volume of the Medico-Chirurgical Transactions. The dissection of the body was conducted by Dr Rainy, then a house physician in the Royal Infirmary, and now Professor of Forensic Medicine in the University of Glasgow; and a second account of the case was communicated by him, when resident in Paris in 1814, to the Medical Society of Emulation there, through which it was published in the 32d volume of the Journal de Medecine of Corvisart, Leroux, and Boyer. Various succeeding writers fell into the mistake of considering these two accounts as having regard to different individuals; and Dr Craigie, in his paper on Obliteration of the Aorta (Edinburgh Medical and Surgical Journal, vol. lvi., p. 427), was the first to point out this error. I should not have re-introduced the case here, had I not had it in my possession, copied from the journals of the Infirmary in its original fulness of daily detail, and also in preparing the foregoing abstract, enjoyed the benefit of Dr Graham's published observations, and of Dr Rainy's private notes.

I should, besides, have been hindered presenting it here, grouped with cases from which it is so pathologically different, but that my chief object, in these communications, is specially to bring into view the distinctive symptomotology of the affections of the opposite sides of the heart. Frere's case accomplishes this to some extent. The obstruction in the aorta in him, I think, notwithstanding the opinions formerly entertained to the contrary, it must be conceded, was congenital, and his immunity from dyspnœa, palpitation, and throbbing of the carotids, so long as the rapidity of the circulation was natural, and the accession of these symptoms in a violent form on the occurrence of circulatory disturbance from the pleuro-pneumonia and pericarditis, illustrate very strongly that latent character, which is so distinctive of sinistral cardiac disease in its early, and that sudden unlooked for tumult of heart symptoms which usher in its concluding stage.

I do not think that the dilatation of the ascending aorta, and the hypertrophy of the left ventricle of the heart, which are reported, form an objection to this view, as I believe that I have had satisfactory evidence that concentric hypertrophy of the left ventricle of the heart, by which its cavity was diminished more than a half, may be constituted in as short a period as ten days.

Case 6.—*Palpitation of heart of some years' standing, but present indisposition of eight months only, and consisting of amenorrhœa, cough, dyspnœa, bloody sputum, and anasarca, with bellows murmur in heart. Death in eighteen days. Extensive atheroma of aorta, and enlargement of heart.*

Margaret Kelly, aged 23, single, a weaver, admitted 19th September 1848. Eight months ago this woman, while menstruating, got wetted, when the discharge was arrested, and has not again appeared; and she was seized with swelling of the legs and belly, which continue. She has had palpitation ever since she can remember, but it has been much increased for some months, attended with difficulty of breathing. Cough, with tough, slightly bloody sputum for three months past, and pain in back. Cardiac space dull for above seven inches across, and two vertically. A continuous soft blowing sound in aorta and axilla, exchanged in the neck and the body of the left ventricle for a harsh continuous sound without any murmur. Skin anæmic; tongue tender; diarrhœa; pulse 104; inspirations rapid. Died on the 8th October.

Inspection.—Strong, old adhesions connected the lower and middle lobes of lungs to walls of chest; right pleura contained twenty-two, and left twenty ounces of bloody serum. Both lungs congested; the right weighed twenty-four, and the left twenty ounces; their upper lobes and general margins emphysematous. The bronchial lining was reticulated, and tubes engorged with frothy serum. Pulmonary vessels natural.

The heart, which weighed thirty-two ounces and a half, was adherent to whole of pericardium by strong, old adhesions, and all its cavities contained fibrinous clots. Endocardium pale, and sinistral valves thickened, contracted, and containing some minute calcareous points. Whole of lower surface of aorta, and of various of its branches, sprinkled thickly by atheromatous matter. This was most remarkable between the heart and the arch.

Peritoneum contained eighteen ounces of clear serum, and adhered to the liver, which weighed four pounds and a half, and was granulated. The gall bladder was filled with whitish coloured bile. The right and left kidneys weighed respectively ten and a dozen ounces; their cortical structure was wholly destroyed, and around the border of the pyramidal bodies were several small calculi. The spleen was much enlarged, and contorted in shape, and weighed seventeen ounces.

As the several parts of this heart were measured with care, the admeasurements, although originating in a previous attack of pericarditis, as much, perhaps, as in the recent atheroma of the aorta, may be here stated :—

Length of right auricle,	3⅝	inches.
.... ventricle,	4⅝	...
...... left do.	4¼	...
Breadth,	3	...
Length of left auricle,	3½	...
Longest diameter of tricuspid opening,	2¼	...
.... pulmonary artery,	1¾	...
.... aortic opening,	1⅜	...
.... mitral	1⅞	...
Depth of aortic valves,	⅞	...
...... pulmonary	⅝	...
Thickness of right ventricle,	¼	...
...... left ...... base,	1⅛	...
.... apex,	¾	...
...... septum,	⅝	...

Case 7.—*Extensive atheroma of whole of aorta and pulmonary artery, preceded by excruciating pain in right hypochondrium, dyspnœa, and ansarca.*

John Morrison, aged 63, a cotton-spinner, was admitted on 8th September 1848. This man states that he had had no serious disease during life, except a fever when a boy, an epidemic purging about seven years since, and an attack of vertigo about five years ago. Says that two months ago was seized with a most severe spasm of the right side, which continued for twenty-four hours, and was followed in a fortnight by distension of the belly, diminution of urine, severe dyspnœa, cough, thin frothy sputum, and in a couple of weeks more by swelling of the feet and legs. All these symptoms continue; abdomen fluctuant. The liver is felt extending from right mammilla to mid-way to umbilicus. The face is swollen, and pale; intelligence very limited; arms emaciated; respiratory muscles much developed; cough increased by dextral decubitus. Respiration of right thorax bronchial and double; but in various parts of left lung it is suppressed, and in posterior inferior parts it is gurgling.

Cardiac impulse gone, and cardiac region sonorous; epigastric pulsation. Heart sounds short, feeble, distant, and frequently dicrotous. Pulse about sixty, irregular, and very feeble. The heart sounds are of a rubbing character; and in the neck, although they can scarcely be perceived, they are entirely diastolic. The jugular veins much distended and pulsating; skin livid.

Urine for last three weeks about a pound daily. Bowels regular. The sputum became purulent and bloody, and he died in a couple of weeks.

Inspection.—The aortic valves were greatly expanded, forming pouches each about seven-eighths of an inch in depth; they were healthy in their structure, but in their immediate vicinity the aorta was encircled by a ring of atheromatous deposit. About two inches farther up the artery its serous coat was very red, and sprinkled by numerous highly-injected or vascular-looking points; and for about an inch of this portion the muscular coat of the vessel was strongly developed. It looked here precisely like a bed of muscle. The circumference of the thoracic aorta was two and a-half inches, and of the abdominal one inch and seven-eighths; and over all this space, and into the femorals, the coats were occupied by numerous pasty depositions, so that a half inch of sound vessel could not be seen. Here and there was a slightly calcareous point, but no bone; and around all the deposits there was more or less vermilion injection of the coats.

The circumference of the heart at its base was 13 inches.

Length of each ventricle	5	...
Stretched diameter of tricuspid	2½	..
.... pulmonary	1¾	...
.... aorta	1½	...
.... mitral	2	...
Thickest part of right ventricle	¼	...
.... left	1⅞	...

All its cavities were much dilated, and their investing lining more opaque and thicker than natural. The right ventricle was covered by fat, and the coronary vessels much enlarged. The pulmonary artery looked as if it had been boiled, and was greatly mottled throughout its various divisions by numerous atheromatous spots. These were seldom larger than a split pea; but they were so numerous, particularly in the substance of the lungs, as to leave little of the surface unoccupied by them. The pulmonary valves were three quarters of an inch in depth. The bronchial membrane was thick and red. The substance of the lungs was darker than natural, but otherwise healthy.

The liver was hard and atrophied. Right kidney weighed four ounces, the left six; congested and granular.

The apparent rapidity of its production, and the small amount of constitutional or local symptoms which sometimes accompany the deposition of atheroma in the aorta, have been conspicuous in some of the cases already related. A few more examples may serve to elucidate the symptoms and progress of this affection more fully, and to exhibit also its effects in the superinduction of hypertrophy and dilatation of the heart, and of aneurism of the aorta.

Case 8.—*A musician, a drunkard; emphysema of lungs, probably from his profession; and jaundice and epilepsy four months before admission, probably from his habits; exposure to cold four or five weeks before admission, followed by chest symptoms and dropsy. Death by epilepsy in three days. Begun atheroma of aorta.*

Robert Barton, aged 22, April 6, 1847. The habits of this man have been intemperate. A month ago he was exposed to cold, and became affected with cough and dyspnœa. About same time feet became swelled. Thoracic symptoms have continued ever since, and anasarca has extended, so that it at present involves every part of surface of body, and there is ascites. The chest appears to be emphysematous, but percussion is damped by the anasarca. Anteriorly the respiratory murmur is very faint and distant; posteriorly it is slightly sibilant. Pulse 96, feeble. There is preponderance and lengthening of first cardiac sound. Urine scanty, highly turbid, but becoming transparent under heat, and not albuminous. Tongue yellowish; thirst; bowels free.

He had compound powder of jalap, and small doses of squill and nitre. On the 9th he had an epileptic fit, to which he now said he had been subject for four months, since an attack of jaundice. He died on the 10th, in about five minutes from the accession of renewed convulsions.

Inspection.—Scalp.—Vessels congested. *Encephalon.*—Vessels of *dura mater* very turgid, and the *pia mater* exceedingly vascular. In each lateral ventricle, on the upper and inner aspect of either *corpus striatum*, were patches of bloody ecchymosis, for the most part of about one-third of an inch in diameter; two or three on each corpus. In the left ventricle, and lying over one of these patches, though quite unattached, was a firm circular clot, of the form and size of a sixpence piece. The ecchymosis was every where confined by the lining membrane of the ventricles, except at one point on the left side, from whence the clot appeared to have been effused into the cavity. The extravasations descended in the grey matter of the *corpora striata* from a line to a third of an inch.

One or two small effusions were seen in the centre of the cerebellum. Except at these parts, the substance of the brain was not more highly vascular than usual, and was of natural consistence.

Thorax.—About a pound of transparent serum in each pleural sac, and pleuræ healthy. Lungs emphysematous, overlapping the pericardium. They were highly elastic, had little fluid in their depending surfaces, and showed various large white patches and dilated vesicles.

The pericardium contained a small quantity of serum, and its cardiac layer was marked by two or three white opaque spots. The heart weighed 17¼ ounces, and from top of right auricle to apex it measured 6¾ inches, and its circumference at base was 10 inches. Much fat was deposited all round upper half of ventricles; on base of left ventricle it equalled one-third of an inch in depth, and the entire thickness of the ventricular wall at that point was 1¼ inch; at apex it was less than half an inch. The right auriculo-ventricular opening was about 1½ inch across, and the left one inch. All the valves, except the aortic, sound; the latter were more opaque and thicker than natural, and there were several white patches of atheroma around root of aorta.

The liver weighed 57½ ounces; it contained much blood, but was healthy in structure. The right kidney weighed 6¼, and left 7 ounces; they also were engorged with blood, but otherwise were healthy. The spleen was dark and pulpy, breaking down with very slight pressure. About two pounds of serum were contained within the peritoneum. Other organs healthy.

In the following case the atheroma of the aorta was associated with an attack of acute rheumatism, and succeeded by an attack of fatal pericarditis.

Case 9.—Duncan M'Callum, aged 34, a dyer, 5th July, 1836. About six weeks ago, after exposure to cold, this man was seized with rigors and pain of large joints, which continue, along with occasional cough, dyspnœa, and palpitation of heart. Thirst severe; pulse 100, full and firm; skin hot. He was put on tartar emetic solution, and four days after he complained much of shooting pains at back of head; three days afterwards he had hiccup, which for three or four days longer resisted all treatment; and on the 15th, ten days after admission, the report is—impulse of heart much increased, and he complains of pain in precordial region, stretching towards left shoulder. Respiratory murmur natural; pulse 120, and throbbing. Tongue furred; occasional singultus. He died on the 17th.

Inspection.—The pericardium contained a large quantity of brownish serum, intermixed with a great many thin flakes of lymph; and both its surfaces were lined with a layer of false membrane, sprinkled over with numerous bloody or vascular points. The heart, especially the left ventricle, was greatly enlarged; the edge of mitral valve was occupied by several small chalky bodies, and those of aortic valves by large warty and cribrated excrescences. The ascending aorta was much thickened, and under its lining membrane there were numerous atheromatous deposits. The right ventricle was the largest, but otherwise healthy.

Case 10.—*A fireman.—Great calcareous deposit in ascending aorta, having its origin in pneumonia of right lung ten months before death.*

Alexander Munro, aged 28, admitted 10th April 1841. This man complained of dull pain in chest, cough, frothy transparent mucous sputum tinged with blood, dyspnœa on the slightest exertion, with occasional palpitation. These symptoms commenced ten months ago with stitches in right breast, and slightly bloody spit. The countenance is blanched; some emaciation; appetite good. Pulsation in right jugular; cardiac impulse strong; pulse regular, but throbbing; a double murmur loudest in heart's diastole heard over chest; much extension of precordial dulness. Respiration bronchial; behind and below, where the thorax is rather dull, the breathing is crepitant. Lower limbs much swollen.

No account of the treatment is preserved; but on the 5th May he is reported to have fallen on the floor while attempting to put on his trowsers, and died.

Inspection.—Head.—Slight effusion under *dura mater;* substance of brain somewhat injected. No fluid in ventricles or at base.

Thorax.—The left pleura contained a considerable quantity of serous fluid. Large bronchia were much reddened, and filled with frothy mucus. The right lung was œdematous throughout, with several blocks of grey hepatization interspersed. The left lung in a similar condition, but less marked.

The pericardium contained nearly a pound of fluid. The heart was enlarged to nearly three times the size of an ordinary fist. Slight hypertrophy, and great dilatation, especially of left ventricle. The aortic valves were thickened, and great calcareous deposit in the aorta, extending about three inches upward.

Case 11.—*Ossification of ascending aorta in a flesher, of above a year's standing. Usual symptoms, with pain of stomach, hoarseness, singultus, and cerebral symptoms.*

Andrew Macmillan, aged 61, 9th September 1819. This man was subject for more than a year to swelling of legs, and for last fortnight to much dyspnœa; hard, distressing cough, especially when he attempts to recline on left side; a sense of suffocation when recumbent, and great increase of dropsy. The stools said to be whitish or reddish; pulse 76, tense; voice hoarse; urine scanty.

The interval was occupied with the use of diuretics; and on 17th October the report is, "œdema and cough greatly diminished. Complains of pain at *scrobiculus cordis* increased on pressure, and relieved by a draught of ether and laudanum. Hoarseness continues." He was brought under the influence of mercury, and on the 9th November was ordered an anodyne draught.

10th, Much drowsiness, and incoherence since taking draught; singultus checked by ether.

The draught was given at 12. Delirium increased; much subsultus, and continued drowsiness, with occasional hiccup. Pulse 84, firm. The mouth is drawn to right side.

A small bleeding was made on the 13th, and on the 14th it is said—

"Twenty ounces of blood taken without obvious effect; a portion of it cupped and buffy. Incessant and violent muscular twitching. He died on the 16th."

Inspection.—Brain.—Considerable milky effusion between the *tunica arachnoidea* and *pia mater*, and a large quantity of serum at base of cranium.

Trunk.—Several broad ossified spots were seen at the root of aorta; the heart was large, but without other change perceptible.

The liver was large and firm, but not otherwise altered in structure; the gall-bladder was very large, and filled with a fluid having little of the qualities of bile.

Case 12.—*Symptoms arising immediately on taking a rapid race on foot ten weeks before admission. Death in six days. Remarkable sanguineous enlargement of the organs; dilatation and ulceration of the aorta; approaching rupture of left ventricle.*

Lockhart Hunter, aged 20, a boatman, August 20, 1842. This man states, that ten weeks ago he ran rapidly a mile and a half, by which he was greatly exhausted, and the day after was affected with palpitation of the heart, pain in the chest, a disagreeable sense of fulness or stuffing about the throat, vertigo, perverted vision, much dyspnœa, cough, and a spitting of florid blood, which last has reappeared usually with the paroxysms of severe coughing and orthopnœa, with which he has continued to be affected. Sleep is often disturbed by a distressing sense of suffocation and syncope, relieved by bending the back forward. The feet and legs have been slightly œdematous during last two days. The anterior surface of the left side of the thorax is dull on percussion. The impulse of the heart is not much increased. A very loud continuous bellows murmur is heard more or less in every part of the chest. In the carotids and femorals it is double. Pulse 112, jerking. The liver is felt extending to near the umbilicus, and the patient vomits all his food. Respiratory murmur nearly healthy.

He was cupped to four, six, and eight ounces from the precordia, and had compound powder of jalap. But he died on the 30th, or ten days after admission.

Inspection.—In each pleural cavity rather more than one pint of reddish serum was contained. The lungs were œdematous, and in their posterior dependent parts somewhat congested, but otherwise healthy.

In the pericardium were about eight ounces of straw-coloured serum. There was great dilatation with hypertrophy of the heart. Its apex was on a plane with the inferior margin of the seventh rib. The left ventricle would have easily held a goose's egg. The auricle of same side was dilated proportionably, and a circular piece of its muscular substance, of the size of a shilling, was found removed from the left of the appendix, the escape of blood being prevented only by a thin transparent membrane of cellular structure.

The aortic valves were thickened and much corrugated, and immediately above these the aorta was thickened in its coats, its lining membrane dotted by numerous white specks, and two or three small ulcerations, and the right half of its circumference dilated, the inner surface of the latter being paler than natural. The other valves were healthy; the openings enlarged, and the outer surface of right auricle roughened by points of lymph. Within the peritoneum was some serum. The liver was of a nutmeg colour. The mucous lining of stomach of a dark red hue from extreme congestion. Other abdominal organs were likewise engorged with blood.

Case 13.—*Symptoms said to be of four months' standing.*

Alexander Erskine, aged 52, a weaver, June 8, 1843. This man has severe cough, slightly bloody muco-purulent expectoration, great dyspnœa, excessive sinking, thirst, and anorexia. He says that complaints began after exposure to cold four months ago. For the last week legs are swollen. The face is livid, the eyes project, and there is tarsal and conjunctival redness. Bronchitic *rhonchi* throughout lungs. Heart's sounds not heard. Impulse visible. Pulse 96, feeble. Liver projects, and belly slightly fluctuant. Death took place on the 10th.

Inspection.—Dilatation of heart, especially on right side, and hypertrophy of left ventricle, were obvious. The aorta was much roughened by earthy deposits throughout ascending portions and arch, and also its valves.

Pleuritic adhesions over left lung. The bronchial tubes of both lungs were red and thickened.

The liver was large, friable, and variegated. The kidneys were pale.

Case 14.—*Two months before admission, and seven before death, symptoms commenced with a cold. Ascending aorta lying in folds from extreme dilatation, with bony deposits, and enlarged heart.*

George Turner, aged 47, a labourer, admitted January 20, 1847. This man, two months ago, after exposure to wet and cold, became affected with cough and shortness of breathing, palpitation, and precordial oppression, with sore throat and hoarseness. He did not confine himself, and three weeks ago became generally anasarcous, for which he used diuretics with much benefit. For greater part of life he has been subject to rheumatic pains of upper extremities; but he never had an acute attack. On admission complains most of the dyspnœa, which prevents his maintaining the horizontal posture for any length of time. Prefers also having the right side lowermost. Respiration hurried and excitable. The cheeks are permanently florid. Cough, attended by expectoration of frothy mucus, having a slightly reddish tinge.

Thoracic parietes prominent. Epigastrium shaken by heart's impulse; precordial dulness extended. A double murmur accompanies the heart's action. This is heard with most distinctness at the right side of the superior third of sternum; that accompanying the ventricular diastole preponderates. Proceeding downwards, the systolic murmur becomes gradually fainter, so that at ensiform cartilage the diastolic is almost alone audible. Over the course of the aorta both murmurs are heard; over pulmonary artery the second only, and that less distinctly. The second is also heard very faintly in left axilla. The systolic sound is soft, the diastolic has a much sharper character. The jugular veins are dilated. Respiration everywhere loud and rough; posteriorly it is accompanied by occasional sonorous and rough crepitant rattles. Ankles are œdematous. Urine in moderate quantity, and not albuminous. Pulse 86, and pretty regular. Complains of weakness; appetite tolerable. Tongue whitish in centre, red at edges; bowels right.

He was put on quarter grain doses of calomel, with nitre, tartar emetic, and opium; dry food; imperial for drink. From the left breast blood was drawn by cupping.

On the 28th the report is, countenance much flushed, drowsiness, and increased force of heart's action, both sounds being now heard in pulmonary artery, and in front of the axilla.

Cupping to be repeated to the extent of eight ounces. To take every fourth hour, until the bowels are moved, half a drachm of compound jalap powder; and to take three times daily two ounces of a mixture consisting of one ounce and a half of sulphate of magnesia, four drachms of nitre, three drachms of cream of tartar, and half an ounce of ipecacuan wine, in one pound and a half of water. Other remedies stopped.

He was dismissed relieved on 22d February, and readmitted March 3d.

After protracted exposure to cold, he was seized with symptoms of bronchitis, pain in right mammary region, anasarca, some hæmoptysis, inability to lie as formerly on right side, and irregular pulse of 88, for which he was blooded yesterday at the arm to ten ounces, and put on calomel and opium, with relief. The blood was buffy, and cupped. The saline solution which he formerly had was repeated, and the calomel and opium continued.

4th, Pain of right side, with a strong pleuritic friction; sound continues. There is cardiac pulsation, with some impulse at right side of sternum. Sinistral decubitus. He was ordered to be cupped to twelve ounces from the lower part of right thorax, and to continue his medicines.

11*th*, Strong pulsation visible in the fourth intercostal space of right side between the sternum and right mammilla, especially when patient is under excitement, and, also, very strongly under fifth rib below the left nipple. Cardiac murmur continues to be heard on the right side of sternum as its focus, especially above the third rib in situation of ascending aorta. At apex a faint systolic murmur only is heard. He was again cupped to six ounces over the heart, and the calomel was stopped, the saline solution being continued; and he was again dismissed relieved on the 8th April, and again admitted on the 21st of the same month, when the report was as follows:—

He states, that on the day of dismissal, he was much exposed to cold, and that in the evening the face, abdomen, hands, and legs became swelled, and the cough, palpitations, and dyspnœa returned. The countenance has lost its former florid tinge. Cough comes on in violent paroxysms, attended by copious frothy expectoration, and sometimes by pain in situation of former pleuritic attack, where a friction sound is still audible. Pulse 84, impulsive. Epigastric pulsation, with some ascites and swelling of liver. Inspirations 40. Pulsations in precordial region, which formerly were visible to the eye, now perceived very indistinctly by hand. Cardiac sounds cannot be perceived, except very imperfectly to left of mesial line of sternum; but towards right side, as far as the nipple, they still appear as an acute, but much lowered double soufflet, the most intense sound being in the situation of second, third, and fourth costal cartilages of right side.

He was put on an allowance of wine, and soon became improved; the heart sounds reappearing, with the increase of strength, on the left side. A small reduction of the stimulus was succeeded by a renewed extinction of the cardiac murmur, except to the right of sternum; and the use of several ounces of gin daily was, in four days more, followed again by return of impulse and of murmurs at left nipple. On the 9th May there was a return of dyspnœa, which yielded to ether; when, on the 14th, two days after the omission of this and the gin, the symptoms seeming to indicate it, four ounces of blood were taken away from the thorax by cupping; and then again on the 19th, the subsidence of the heart's power appearing to demand it, he had once more a little wine to the day of his death, which took place on the 8th of June.

Inspection.—The body was emaciated.

Thorax.—Both lungs were bound down nearly all over by firm adhesions. The right pleura presented a large, but limited, and well defined empyema, which contained a wash-hand basin full of purulent matter and serum. The right lung was of extremely small bulk, and carnified. The left lung was congested.

The pericardium contained about four ounces of serum, and the heart greatly enlarged. The walls of the left ventricle were about three-fourths of an inch in thickness, and capable of containing about six ounces. The aorta lay in numerous flaccid folds, from its previous extreme dilatation; and its coats were unusually altered by atheroma, interspersed with ossific plates. Each auricular opening allowed the passage of four fingers. The left valves were a little thickened. The weight of the heart was twenty-three ounces.

The weight of the liver was forty ounces; that of kidneys six ounces each; the former in a state of granular degeneration, and in one or two places much congested; the latter large from engorgement.

It is to be regretted that the constitution of the infirmary, and the crowding in of patients in winter, made it necessary to dismiss Turner. His life would have otherwise been protracted. But the case is interesting, as exhibiting under our daily inspection the several steps of its fatal progression. The empyema was doubtless the immediate cause of death.

In the next case the symptoms were of four months' standing on admission, and of six at death. His death was the result of copious effusion into the pericardium, probably accelerated by the effects of digitalis.

Case 15.—Hugh Crooks, aged 60, a calico-printer in the country, 19th April 1848. This man, four months ago, after a wetting, was seized with cough, pain of chest, fits of dyspnœa, and viscid expectoration, which, during last month, have confined him almost wholly to bed, and for the past week have been complicated by dropsy of lower limbs. Pulse 100; urine natural in quantity, but very albuminous. The liver is felt enlarged. Precordial dulness, especially behind sternum, considerable. Mucous rattles. A loud continuous bellows murmur in heart; the systolic portion of the sound being best developed over the left, and the diastolic over the right side. Apex of heart is to left of left nipple. The face pale, and lips sublivid.

He was cupped over the epigastrium to six ounces; a fomentation was applied to the same part; he was put on a solution of muriate of ammonia and nitre, with taraxacum, ipecacuan, and rhubarb; and he had small doses of calomel, squill, and opium, with the occasional use of the hot foot bath. He went home, at his own request, on the 4th May, relieved of the pulmonary symptoms, the report of the cardiac being as follows:—

Lips continue sublivid. A line of dulness extends from dextral edge of left mammilla three and a half inches to the right. Former bruit, in which the sounds were merged and continuous, is now reduced to a double murmur of a blowing kind heard best over the aortic valves. The diastolic sound is at present most remarkable along aorta and cervical vessels, and the systolic in the vicinity of the axilla or mitral orifice. Much throbbing and starting, particularly when the patient is excited, of the brachial and ulnar arteries.

May 31*st*, A week after leaving the house, caught cold, when all his former symptoms returned. Along upper part of left ventricle, and whole of aorta and *arteria innominata*, the systolic sound is normal, and the bellows murmur is confined to the cardiac diastole; but under, and to the outside of the left nipple, there is a large, well developed, and soft bruit in both sounds of the heart, with a distinct interval interposed. Dyspnœa severe; legs swelled; loud, dry, expulsive cough.

He was put on small doses, repeated every fourth or sixth hour, of calomel, squill, nitre, ipecacuan, and cream of tartar; and on the 3d June he was ordered to take of the following mixture half an ounce every third hour:—

Infusion of digitalis, three ounces; spirit of nitrous ether, tincture of gentian, of each half an ounce; mixture of carbonate of ammonia, seven ounces.

7th, Pulse at wrist 72, and consists of a long and short beat alternately, or, should the length of two consecutive pulsations be equal, they are divided by a very long pause. At the heart the beats are dicrotous, or sometimes more, the whole beat being made up of a lengthened continuous stroke, divided into three smaller portions. Urine three pounds and a half.

To have daily three ounces of gin; the mixture of infusion of digitalis to be continued.

On the 10th, the anasarca had disappeared, although not the dyspnœa; and the digitalis and gin were omitted. On the 15th, a diminution of the urine led to their resumption. On the 21st, the dropsy being considerable, and the urine still scanty, the digitalis mixture was increased to half an ounce every two hours. On the 22d, he expressed himself as being greatly relieved, and his urine increased. His manner in reference to this was peculiarly energetic; and, on the night of the 22d, he stated to the nurse that his rest had been most refreshing; but in the morning he was found dead in the water-closet.

Inspection, thirty-six hours after death.

Head.—About four ounces of dark blood dripped from brain on its dissection. The *dura mater* was firmly attached to the bones, and the brain was highly congested, but otherwise healthy.

Thorax.—Some old, and also recent pleuritic adhesions on both sides. Lungs weighed each thirty-three ounces, and the larger bronchia were inflamed. The pericardium contained seven ounces of serum; and the heart and great vessels, when washed, weighed thirty-three ounces avoirdupois. Its admeasurement from

Apex of right auricle, to apex of left ventricle, was 8 inches.
And across at base, ... $4\frac{1}{2}$ ...
Average thickness of walls of left ventricle, ... $\frac{3}{4}$ ...
.... of right .... $\frac{1}{4}$ ...
.... of each auricle, ... $\frac{1}{8}$ ...

The walls of the right ventricle were soft, while those of the corresponding auricle were uncommonly dense, and both were almost wholly covered by fat. This deposition on the left side was limited to the course of the proper vessels of the organ. The left ventricle might easily contain about a breakfast-cupful of fluid, and all the other cavities were similarly dilated. The right auriculo-ventricular opening allowed the presence of five, that of the pulmonary artery of three, and that of the aorta of two, fingers laid parallel; in the two latter with some stretching. The mitral valve and orifice had much the shape of a canal. This appearance was produced partly by the yielding and inclination toward the ventricle of the walls of the auricle immediately around the opening, and partly also by the strength and increased size of the curtains of the valve, as they stood out thickened and opaque into the cavity of the ventricle. The circumference of the aorta, about two inches up from its orifice, was four inches and a half. Its coats were greatly thickened, opaque, and uneven, being mapped out into numerous slight linear depressions, by the copious deposition of atheroma under its lining membrane. The sinuses of Morgagni in particular were enlarged, and their parietes roughened by osseous plates, and the opacity and thickening extended, but in diminished degree, to the valves and endocardium.

Abdomen.—Weight of liver, 3 pounds $9\frac{1}{2}$ ounces, and much congested. Spleen weighed 8 ounces, and kidneys each $7\frac{1}{2}$ ounces. The latter were granular.

In the succeeding case, the time required to run its course was

two years; and it was associated with paralysis, and the presence of what had originally been a blood clot on the *pons varolii.*

Case 16.—Janet Bell, aged 67, June 2, 1848. This woman, about two years ago, had general rheumatism, accompanied by palpitation of the heart, and succeeded in six months by palsy of the left side, and, six months ago, was seized with dyspnœa, cough, and swelling of the feet. Visible impulse of heart extends from third to sixth left rib. A continuous plane sound is heard over left ventricle and aortic orifice, obscured in other situations by bronchitic rhonchi; although, in the intervals of the dyspnœa, it may be heard also in the vicinity of the mitral opening. In the ascending aorta and neck there is only a louder and more acute second sound than natural. Jugular veins much enlarged. Pulse 104, and feeble. Appetite good. Urine scanty, of specific gravity 1028, and highly albuminous. She had three ounces of gin on admission, and on the 5th, when I first saw her, she was put on squill and nitre, with two drachm doses of cream of tartar morning and evening. She died on the 7th, having previously had some wine prescribed.

Inspection on the 9th.—The brain was remarkably pale, and its veins empty. About one ounce of serous effusion at base, and a small quantity within the ventricles. At the mesial line of the *pons varolii*, at the point of junction of the motor and sensitive columns, a rust-coloured spot, of the size of a sixpence piece, was observed in the substance of the brain.

Thorax.—Old adhesions of the right lung to pleura, and of its lobes to each other. The bronchial lining of large tubes much congested; that of smaller healthy.

The heart weighed 16½ ounces. Right ventricle covered with fat; left increased in thickness of walls to 1½ inches, and cavity could contain no more than a hen's egg. Valves of aorta healthy. The free edge of the mitral was converted into a mass of soft tubercles, of the size each of a barleycorn. They were not fungous growths, but simply cauliflower-like extensions of the natural tissue of the valve; and the fleshy columns here and in opposite ventricle, and also the appendices of both auricles, were all more fleshy and massive than natural. Tricuspid opening allowed three fingers, pulmonary and mitral each two, and aortic the index finger only, to be passed. Aorta itself dilated on its right side, just as it turned to form the arch, and here measured 1¾ inch diameter. Its coats were thickened, discoloured, and fissured by numerous atheromatous deposits, the surfaces of which were cribrated here and there by minute openings.

Abdomen.—The liver was anæmic. The kidneys were mottled. Omentum and mesentery much congested.

Case 17.—Andrew Nielson, aged 32, formerly a musician, now a porter, 27th April 1812. Has had slight palpitation of the heart for two or three years; about a year ago began to have dyspnœa, a sense of heat and oppression at heart on exertion; and, six months since, was seized with pain under sternum, severe cough, particularly at night. Other symptoms continue. Countenance anxious, temper irritable. Pulse 100, and regular. He was blooded to 15 ounces, with relief, and three nights after, he had two grains of opium. He was suddenly seized during the night with extreme dyspnœa, and died in a few minutes.

Inspection.—A considerable portion of osseous matter was deposited all round the root of the aorta, the valves of which were imperfect, and its arch dilated to twice its natural size, but without any rupture of its coats.

Case 18.—Angus M'Callum, aged 36, a shoemaker, May 16, 1848. Fifteen months ago this man was seized with severe pain in the right infra-clavicular region towards the sternum, shooting up into the shoulder and back part of the neck, that in former situation being aggravated by dextral decubitus. This pain was mitigated, but not removed, till about two months ago, when it became worse, and he first perceived a swelling in the situation of the sternal pain, which has gradually increased till now, when it is of the size of a thin segment of an orange. This tumour is in the situation of the arch of the aorta. It imparts a very sensible thrill to the finger when touched, and this movement extends over a good portion of the chest around. The swelling is the seat of a systolic bellows murmur, which is also heard behind, about the left side of the fourth dorsal vertebra, and for a little way upwards and downwards from this point. He states that he was attacked with cough and great dyspnœa, with tough expectoration, on the first appearance of the tumour, which continue; that he has occasional violent palpitations of the heart even when at rest in bed, but especially on first assuming the erect position; and complains of a sense of heat, like to that which might be occasioned by a stream of hot vapour across the thorax in the line of the tumour. Cough husky and tracheal; slight swelling of left face from constant lying on that side. Some pain in region of kidneys, frequent micturition, and discharge sometimes with urine of a little blood. Pulse 70.

After a few days' residence in the house, there was a sensible increase in the bulk of the aneurism, and the patient having expressed a wish to return home, he was dismissed.

Case 19.—Mary Smith, aged 40, a cotton picker, admitted 3d February 1838. This woman says, that she has had cough and expectoration for about a month, but that within a week she was seized during the night with sharp pain in the right shoulder, thence darting towards heart, where the pain continues, accompanied by dyspnœa and much oppression. The countenance is anxious. A puffy swelling around the shoulder is the seat of much pain on pressure, and towards the middle of the sternum a flat, soft, elastic tumour, of the size of the palm of a small hand, has recently appeared. Much uneasiness is caused on the pressure of this tumour. It is very dull on percussion, and, on using the stethoscope, the heart sounds are heard obscurely, accompanied by a distinct bellows sound, which latter is also perceived at the heart. She states that she never had rheumatism or palpitation till the date of present illness. The right side of face is the seat of puffy swelling. Pulse is weak; tongue clean and tremulous; urine scanty; sleeps ill. She had some simple treatment, and died unexpectedly on the night of the 11th.

Inspection.—On removing the sternum, the pericardium was seen in its natural position, containing some fluid, a flake or two of lymph, and a rather small heart, the surface of which appeared tuberculated in consequence of the peculiar disposition of its fat. A globular tumour of the size of the fist lay under the top of the sternum, and adhered to it, while to the outside of the tumour the right lung was fixed by cellular connections. The right pleura contained about two pounds of clear greenish serum, and a quantity of coagulated blood; on removing which the lung was seen to be pressed upwards, and to be slightly compressed, and the internal surface of the sternum to be roughened. Just at this point the aneurismal sac had given way, apparently from a gradual thinning of its walls. The irregularly torn opening was obstructed by firm fibrinous clots, which, on laying the tumour open, were found to line its interior to the thickness of about one inch. The sac itself was of the size of a large orange. It lay upwards and outwards from the arch of the aorta, and the large trunks were given off just as the vessel, suddenly emerging from the sac and contracting, turned downwards, to come in contact with the spine. The posterior wall of the aneurismal sac was formed by the very broad and atheromatous ascending aorta, and the anterior by the distended outer coat of the vessel, lined by the fibrinous laminæ formerly mentioned. The aortic valves were slightly thickened.

2d Series.—Principal disease in the aortic valves.

Case 20.—*Cardiac symptoms observed only for six weeks. Evil effects of physical exertion, also of bleeding, and again of depressing moral causes. Advantages of stimulation. Death finally from exhaustion produced by diarrhœa.*

William Shields, aged 23, a collier, August 2, 1848. About seven years ago, this man had rheumatic fever, but without cardiac symptoms. Six weeks since he was seized with pains in knees and ankles, which continued for a few days, and were succeeded by pain and palpitation of heart. Although suffering from these symptoms, he continued to work partially till a fortnight ago, when he began to be breathless, and to complain of a sense of general soreness in the heart. He had been brought in a cart a distance of six or eight miles, and was much exhausted. Pulse at wrist about 150, and at heart 190. Pulsations of heart visible at ensiform cartilage, also under the fifth rib to the left of nipple, where there is extended dulness and strong purring. Heart's sounds single, and consist everywhere of a mixture of ringing and rubbing sounds, the latter predominating towards the left axilla, in the pulmonary artery, and in the aortic orifice, in the order of their names. The jugular veins were turgid, with some pulsation; much throbbing of carotids, subclavians, and of abdominal aorta; percussion of general thorax good; respiration loud and bronchial. Face and head sweat much; hard cough; carbonaceous sputum sometimes streaked with blood; feet œdematous; cannot recline, except with shoulders raised. Face and lips anæmic; bowels slow; urine high coloured, and of sp. gr. 1013, not albuminous.

He was ordered to take, three times daily, or oftener if requisite, two ounces of a mixture containing twenty-four grains of sulphate of quinine, half a drachm of sulphate of iron, one ounce and a-half of sulphate of magnesia, one drachm of aromatic sulphuric acid, in twenty-four ounces of peppermint water; to use the warm hip-bath twice daily for half an hour at a time; to take a dose of the diuretic powder, without foxglove, every six hours, and to use imperial for drink.

5th, Symptoms much moderated. Medicines to be continued.

6th, A temporary house physician having made the visit last night, and observed the violent pulsations in the cardiac and epigastric regions, ordered the application of leeches, which bled profusely. Patient much sunk; skin jaundiced; breathing panting and very laborious; pulse cannot be counted with accuracy; three stools.

To have every hour one ounce of the carbonate of ammonia mixture, and to have four ounces of wine.

7th, Patient was this morning subjected to great mental excitement and distress, and is at present in a state of profound exhaustion, with sopor. Expirations, which are forty per minute, are loud and laborious. Pulse at wrist 140; at heart 150, with loud rasping sound. Hands œdematous.

To have one ounce of wine every hour till evening, and afterwards according to symptoms.

8th, Has had about eight ounces of wine since visit. Pulse 148 at heart, 120 at wrist. Face much shrunk; two stools; sopor continues.

To take every second hour half an ounce of wine, and to have every fourth hour an injection of beef-tea. To take chalk mixture with catechu if symptoms require it.

12th, Pulse 80 at heart and at wrist. Rasping character of heart sounds less obvious; feelings much more comfortable; sopor less, and can now retain the horizontal position. Complexion improved; diarrhœa abated; expirations 32, and not so forcible.

Medicines to be continued.

14th, Seven stools; bitter taste in mouth; pulse 84; liver hard and projecting. A strong continuous bellows murmur is heard from left nipple to cartilage of fourth corresponding rib. In other situations, the sound is the natural one, a little louder than what is normal, or somewhat ringing, but without murmur. In the neck, it is a loud continuous rasping sound.

A blister to be applied to the epigastric region for three or four hours. Wine to be continued.

15th, Some flushing.

Wine to be diminished to six ounces daily. To have every sixth hour one dose of the diuretic powder, with foxglove.

17th, Pulse 104; strong.

Wine to be reduced to four ounces daily. Powders to be continued.

21st, Violent tenesmus, with purging of spinage-coloured stools. Was much exhausted by fruitless efforts during the night to evacuate the bowels, and has been seized since morning with a return of the palpitation. Pulse at wrist 120, at heart 188. On the succeeding days, and while using stimulants largely, the powers continued to fail, the jugular pulse disappeared, and he died on the 25th.

Inspection.—The pericardium contained about eight ounces of serum, slightly tinged with blood. Heart weighed seventeen ounces.

Diameter of tricuspid opening,	2	inches.
... of pulmonary artery,	1⅜	...
... of mitral opening,	1¼	...
... of aortic opening,	⅞	...
Thickness of wall of left ventricle,	¼	...
... of septum,	¾	...
... of wall of right ventricle,	¼	...

Aorta occupied by patches of about the size of a split pea, of putty-like deposit, as far down as after giving off of left subclavian and its valves contained several bony bodies of about the size each of a barleycorn. About thirty-six ounces of bloody turbid serum were contained within the pleura. The lungs were filled with carbonaceous matter; the pleura pretty generally adherent, some blocks of hepatization, emphysema of upper lobes, and bronchial lining pink coloured.

The liver covered by some flakes of lymph, and of a dark yellow colour. Weight two pounds fourteen ounces; gall-bladder filled with viscid bile. The kidneys, each weighed six ounces, and especially the right had begun to be degenerated. The spleen weighed six ounces.

Case 21.—*Increased width of aortic orifice and bony deposits in its valves; cirrhosis of liver, and death from serous apoplexy. Case at first resembling dextral disease.*

David Wylie, aged 56, a labourer. This man has frequently had attacks of general rheumatism, confining him for a day or two to the house, and has been subject also to catarrhs, and to vertigo and acute headach; but says that, with the exception of a slight occasional cough, he was in good health till about two weeks since, when abdomen and lower extremities began to swell. During eight days has suffered from shortness of breath and severe cough; face and eyes suffused; decubitus chiefly on back.

The jugular veins are as large as the finger, but without pulsation. The thoracic parietes are prominent; percussion generally clear, and precordial dulness slight; the cardiac impulse weak, and best felt above nipple, apparently from great abdominal inflation; breathing oppressed, tracheal, and wheezing; vesicular murmur in most places harsh, and often accompanied by sonorous rhonchus; considerable preponderance of first cardiac sound, which has at the same time a peculiarly abrupt and sonorous character; sleeps ill; much thirst; tongue large, red, and dry; appetite good; bowels regular; urine scanty, sp. gr. 1·030, and slightly but distinctly albuminous; is subject to wandering pains over body, and has some difficulty in articulating, and slight loss of power in arms.

The treatment consisted of free purgation, and of solution of iodide of potassium with tincture of cantharides as a diuretic, cupping and warm fomentations over loins, and warm hip-bath. On the 12th of January he was passing four pounds of urine daily, and on the 20th he complained of increased general soreness, inability to move; the skin became dry, cold, and livid, the urine suppressed, sopor and insensibility came on, and he died comatose on the 23d.

Inspection.—Head.—Arachnoid cavity contained about two ounces of clear serum; its vessels were very turgid, and on its cerebral layer at upper surface of brain was extensive deposition of white, opaque, and firm matter, apparently lymph of old standing. Little fluid was contained within the ventricles, and substance of brain was natural.

Chest.—The lungs were highly emphysematous, completely concealing the pericardium. Their dependent portions contained much fluid; no hepatization or tubercle. The pleural laminæ were free from adhesion.

The heart was dilated in all its cavities, with increase of substance. It weighed fourteen ounces. The aortic orifice was very wide, and a small rough bony mass was found on the upper border of two of the valves, the larger being about the size of a pea. The mitral orifice measured one inch and a half in diameter. The structure of valves was normal. The pulmonic valves were healthy, and retained fluid poured on them from the artery The tricuspid orifice was one inch and a half across.

Abdomen.—Much gaseous distension of intestines, and about six pounds of peritoneal effusion of a straw colour. The mucous lining of stomach was pale and granular looking.

The liver was contracted, and its weight only thirty-one ounces. Its substance was studded with masses of a fawn colour and somewhat soft consistence, contrasting strongly with the surrounding structure. The altered portions varied much in size; they were generally oval or circular, and one in the left lobe was as large as a hen's egg, others were small as a hazel nut, and where they reached the surface they formed projections which gave to the general surface a nodulated feel and a variety of colour. The section of these masses was smooth and uniform, but they were readily broken down in their tissue. Bile was viscid and of a dark brown colour.

Spleen was moderate in size, softened, and almost black. The right kidney was absent; that of left side was much hypertrophied; its weight eleven and a half ounces, and the gland was in the early stage of granular degeneration.

Case 22.—*Violent paroxysms of dyspnœa, cartilaginous degeneration of aortic valves. Death in about six months from first seizure.*

James Ramsey, aged 40, 28th November. This man was exposed about six months since, when in robust health, to excessive fatigue, after which he began to be affected with his present complaints. The most trivial exertion, pressure on the liver, or any attempt to recline, induces a sense of tension in the precordia, and laborious breathing. The stomach is distended by flatus; he has much wheezing cough, coming on with anything which happens to diminish the capacity of the thorax, and it is often relieved by discharge of air from the stomach. Has diarrhœa; strangury; œdema of feet; alarming paroxysms of dyspnœa, particularly in the night; frequent and deep sighing; countenance sallow, and during the dyspnœa livid; pulsation of heart very indistinct; pulse 96, labouring, and with a jarring throb. Pants and gasps on any attempt to speak, and sometimes expectorates clotted blood. Urine of a yellowish red colour, with a pale, pink precipitate.

Much watchfulness, pain near the left mamma, and often dreadful sense of oppression about the heart. Death took place on the 10th December.

Inspection.—The heart was nearly double its natural size; an ounce and a half of pale serum in pericardium. The right side of heart, the auricle especially, was greatly dilated. The aorta was greatly distended by dark blood, its valves puckered, thickened, and cartilaginous. Lungs and abdominal organs engorged with blood.

Case 23.—*Terminating by convulsions about eighteen months after commencement of dyspnœa. Death preceded for two months by slight hemiplegia and painful ecchymosis of legs.*

Robert Burns, aged 19, 15th November, 1837. This person has been subject for eighteen months to dyspnœa and palpitations, and within last eight weeks to œdema of legs. Five weeks since had slight palsy of right side, the gastrocnemii of which have continued painful, and the skin on both legs ecchymosed extensively. Left thorax is about half an inch more by measure than right, and the mammary and infra-mammary regions are very dull. Impulse of heart strong and heaving, and its sounds marked by a remarkably loud rasping systolic murmur. Pulse 132, full and thrilling. The carotid arteries throb. Crepitus universal over thorax; sputum purulent, and sometimes bloody; breathlessness most distressing. Countenance very sallow; tongue slightly white; appetite poor. Blood was drawn from the arm, and in the course of 24 hours afterwards from precordia, each time to sixteen ounces, with relief to urgent symptoms. The blood drawn from the arm was buffy. On the 18th he had infusion of foxglove (half an ounce three times daily); and on the 24th, anasarca all but gone; urine five pounds, very dark. Cough and dyspnœa much diminished, especially during day. Some uneasy sensations in right arm, and complains for two days of beating pain in right temple. Action of heart increased, and more jarring. Tongue pale and moist; thirst; bowels open with cream of tartar; pulse 80, and sometimes intermits.

Foxglove infusion to be stopped.

On the following day (25th) he complained of intense pain in the heart, throbbing in the head, and dyspnœa, for which fourteen ounces of cupped and buffy blood were taken from the arm; a blister was applied; and opium and nitrous ether given. On the 28th, the symptoms having remained, he became convulsed, and died in an hour.

Inspection.—Head.—A large quantity of fluid effused under arachnoid, and in the theca of the spinal chord. The vessels of *pia mater* were very turgid, and its tissue infiltrated by serum. The whole surface and substance of brain had a uniform rose-coloured tint, an unctuous feel, and firm consistence; and on making an incision into the upper convex portion of left hemisphere, where a slight depression and bloody tinge of the surface seemed to indicate extravasation, the scalpel fell into a cavity capable of containing a pigeon's egg, and nearly filled with a coagulum of blood and softened brain. The upper wall of this cavity was about one line from the surface of the hemisphere, and its lower about a quarter of an inch from the roof of the lateral ventricle.

Thorax.—The pericardium was much enlarged, and contained about fourteen ounces of clear straw-coloured serum. The heart was fully twice its natural size; the walls of the left ventricle were from an inch and a-half to two inches in thickness, and both sides, particularly the right (the parietes of which were also thicker than natural), dilated. The aortic and mitral valves were very dense, thickened, and cartilaginous; and the free edges of the former were eroded and studded by warty excrescences, which also extended within the ventricle. Coats of aorta very attenuated, and diameter of the artery at no part exceeded three-fourths of an inch, and beyond the arch it was less.

The lungs were of large size, heavy, everywhere slightly consolidated, and in some parts almost fleshy.

The lobular and interlobular tissues of the liver were highly engorged. The spleen immensely enlarged, lobulated, and of remarkable firmness and density. In several of the interstices of its lobules, and in its substance, were deposited firm masses of a yellowish white cheesy substance.

The kidneys were large, and generally of a greenish colour, intersected by yellowish bands of a granular texture.

Case 24.—*Cough of seven, and œdema of legs of two months' standing, accompanied after this by pain above* umbilicus; petechiæ, *and after venesection; by epistaxis; palpitation, and death. Aortic valves ossified, spleen, greatly enlarged, but soft.*

David Turnbull, aged 29, 28th June. The feet and legs of this patient are œdematous, and belly is swelled, tense, and painful. Former is of two months, and latter of ten days' duration. Belly indistinctly fluctuant. Has had cough for seven months, exciting pain in breast, and attended with expectoration. Pulse quick and sharp; face sallow; frequent flushings; sleeps ill; tongue moist; belly regular; urine scanty. Was put on half an ounce of infusion of digitalis three times daily.

30th, Pretty sharp pain on pressure just above navel; numerous petechia on legs. He was blooded to twelve ounces, blood giving a soft coagulum. Diarrhœa appeared on the 1st, epistaxis and palpitation on the 3d July. The latter symptom continued, with whizzing intermittent pulse, till the 7th, when he died.

Inspection.—Four ounces of water in pericardium; whole heart was much enlarged, and aortic valves partly ossified. Spleen four times its natural size, and of soft consistence.

The abdominal viscera were much engorged.

Case 25.—*Cough and dyspnœa after a cold, followed by albuminous dropsy and numerous petechiæ; treated by frequent blood-lettings. Death; softened heart, gritty aortic valves, pulpy spleen, and granular kidneys.*

Robert Raeburn, a carrier, 25th September. Long an habitual moderate drinker of ardent spirits, and in January last threw off a greatcoat which he was accustomed to wear, and was affected for months with cough, stifling in chest, and debility, and two months ago by general anasarca, which continues, interspersed by petechiæ of various sizes. The belly is fluctuant; he lies indifferently on either side; thirst excessive; urine high coloured, made frequently during night, and in considerable quantity, sometimes with scalding. Sharp dry cough; appetite good; diarrhœa; complains much of pain in right iliac region; pulse 104, full.

He was blooded on the 1st of October, and drowsiness coming on on the 8th, the bleeding was repeated on the 9th, and again on the 10th, and he died on the 11th.

Inspection.—The cuticle was raised extensively into large vesications. The brain pulpy, and its cavities were occupied by serum. Cavities of trunk were each the seat of considerable serous effusion.

The pericardium contained four ounces. The heart was attenuated and soft; its cavities dilated, and aortic valves gritty.

The liver was pulpy; spleen resembled clotted blood; and kidneys also softened and much mottled.

Case 26.—*Chronic albuminuria; aortic orifice filled with warty growths.*

Jean Frazer, aged 40, 2d November. Complains of general debility and loss of appetite, distension of abdomen after eating, and uneasiness in hypochondria. Percussion of thorax pretty clear; that of right hypochondrium more extendedly dull; natural; some bronchitic rattles; respiratory murmur faint in lower anterior parts of lungs; sounds of heart regular, but very loud, and heard in right side; pulse 90, throbbing; face sallow; skin hot and dry; bowels slow; tongue white and clammy; urine, sp. gr. 1010, and slightly albuminous; anasarca of lower extremities; and latterly, dyspnœa and great exhaustion; death on 3d December.

Inspection.—The valves of the aorta were converted into warty growths, filling up the opening. The left ventricle was slightly hypertrophied.

The liver was much enlarged from simple congestion.

(*To be continued.*)

ART. II.—*On the Nature and Treatment of Spinal Affections, usually termed Cases of Spinal Irritation.* By GEORGE PATON, M. D., Wetherby, Yorkshire.

THERE is a class of complaints, termed cases of spinal irritation, which have of late received considerable attention from the profession. They are characterised by pains in various parts of the body, as the chest or abdomen, weakness in the limbs, shortness of breath, and inability to undergo exertion. Sometimes there is pain of the head, with giddiness, nausea, and pain at the stomach, with vomiting of food after meals, palpitation of the heart, and other distressing symptoms, which seem to simulate almost every disease to which the human body is liable. Not unfrequently, after the disease has existed for some time, weakness may be complained of in one or more portions of the spine, which may be attended with nothing more than a slight uneasiness after walking or exercise; and on examination being made of the part, acute pain and tenderness is generally manifested on pressure. What may be the nature of this disease, or the state of system under which it is developed, is a question of much importance to determine, and respecting which various opinions have been advanced.

By the physicians of this country, the disease has in general been considered as connected with the function of the spinal chord; hence the term Spinal Irritation has been applied to it. And the opinion most recently advocated is, that the pathological cause of the disease is congestion of the chord,—that, the veins of the spinal chord being destitute of valves, congestion is liable to take place in them in weak and cachectic subjects, and give rise to the various symptoms indicated in this affection, on the physiological principle, that pain is often felt at the extremities of the nerves, when the disease is seated at their origin, or centre of the nervous axis. But many of the French physicians believe that these affections have no connection with the functions of the spinal chord, but are dependent on the state of the general system, and to be treated with remedies directed to this end.

If, however, we attentively examine the symptoms manifested in these affections, in connection with what we know of the physiological action of the spinal chord, and employ the same mode of reasoning in this as in every other science, in tracing an effect to its particular cause, there can be little doubt that the function of the spinal chord is more or less involved in these diseases. When we find acute pains referred to particular parts of the chest or abdomen, where the existence of no positive disease can be detected, or the limbs affected with great loss of power and diminished sensibility, and apparently no indication of any morbid state in the brain to account for the symptoms; and when we perceive that, as a portion of the spine corresponding to the origin of the nerves which are distributed to these parts of the body, there is weakness complained of, with acute pain and tenderness on pressure; we are led to conclude that the cause of these complaints is connected with the functions of the spinal chord,—that the affection is of a spinal character. Accordingly, as a particular portion of the spine is affected, we find a corresponding set of symptoms. If the disease be in the cervical portion of the spine, the complaint follows the course of these nerves. There are pains stretching along the neck to the shoulders and fore-arms; or there may be weakness in one or both upper extremities. If the disease be in the dorsal region of the spine, we find difficulty on inspiration, shortness of breath on exertion, with acute pain stretching across the chest, frequently referred to the sternum or epigastrium. And if the lumbar region be affected, we have pains extending across the abdomen, with difficulty of micturition, and pains and weakness of the lower extremities. These are facts which a knowledge of the functions of the voluntary system of nerves enables us to explain, and to consider as resting on admitted data.

And perhaps the recent investigations of Valentin may throw additional light on this subject, and explain how some of the organic functions of the body may be influenced in these affections. This inquirer has shown, by numerous experiments, that irritation of the spinal chord affects the movements of the œsophagus and viscera of the abdomen, through the medium of the fibres of the sympathetic system of nerves; that in this manner contractions are produced in the stomach by irritating the roots of the lower cervical and first thoracic nerves; and that, by irritating the roots of the dorsal nerves, or of the lumbar or sacral, corresponding portions of the intestinal tube are affected. In short, the researches of Valentin show, that by means of the communication that exists between the fibres of the spinal and sympathetic system of nerves, irritation of the spinal chord affects the movements of the viscera of the abdomen. It is very probable, therefore, that on this principle may depend much of the pain and suffering connected with these functions of the body in diseases of a spinal character,—that irritation at the origin of the spinal nerves, communicated through the medium of the sympathetic, may influence the functions of the stomach and other abdominal viscera, and produce many of those symptoms which we witness in these affections.

One circumstance, then, particularly to be attended to in this class of diseases, is, that the most prominent and characteristic symptoms may be manifested in various parts of the body at a distance from the seat of the affection. Thus pains are felt at the extremities of the nerves, whilst the cause is connected with their origin; and the symptoms often appear so anomalous as to prove most perplexing to the physician, whilst, at the same time, they are most distressing to the patient. The sensibility, in some parts of the chest, may be so much increased, that the patient shrieks when it is touched, and can scarcely bear the pressure of her clothes on it. The integuments swell, and become acutely painful even on the pressure of the finger. There is often great pain, with irritability of stomach,—pains and attacks like spasm in the bowels, accompanied with dysuria. There may be a feeling of tightness extending along the margin of the ribs, and affecting the power of respiration; or there may be weakness, with a sort of prickling sensation in one side of the body. In some cases, which the author of this paper investigated in the French hospitals, sensation was greatly impaired in one side of the body, whilst the motor power remained unaffected. In others, sensation appeared to be diminished only in a small part of the body, as in the integuments of one side of the chest, whilst the patient complained of general lassitude and feebleness in the movement of the limbs. These are some of the symptoms of cases of spinal irritation which are frequently to be met with in practice, and are liable to be confounded with other diseases, or considered of a local character. But the symptoms do not readily yield to the remedies employed till attention is directed to the spine, when, by counter irritation and other appropriate treatment, the complaint, if taken in its early stage, will generally be subdued.

With regard to the pathology of this disease, in considering it to consist in congestion of the spinal cord, we confess we cannot acquiesce in this view of the subject. We do not see how congestion of the cord should give rise to the acute pain and tenderness on pressure of the spinal column that is often manifested in these cases; more especially, since it is a well known fact, that in inflammation of the spinal cord, pain often cannot be produced by pressure of the spine.* And the course of the veins of the cord renders them protected from all external influence, and equally from pressure applied to the vertebral column. Besides, from the intimate communication which exists between the spinal veins throughout the whole course of the spinal canal, and the free communication which they have with the *vena azygos* and thoracic veins, &c. of the trunk through the intervertebral foramina, it is difficult to perceive how congestion should take place at one particular part, to such an extent as to interfere materially with the functions of the spinal cord. And if congestion did affect one portion more than another, it should be the lowest and most dependent portions, where the circulation is most under the influence of gravitation, and where the greatest force requires to be exerted to propel the contents of the vessels forwards. But in these affections we do not find the lumbar portion of the spine more affected than the other portions.

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* Abercrombie on Disease of the Brain and Spinal Cord, page 381.

Besides, one principal reason why these veins are destitute of valves, is evidently to prevent congestion from taking place at particular parts of their course, and any injurious effects resulting to the functions of the cord. It is a well known fact, that during the act of inspiration the progress of the blood is accelerated towards the heart, and slightly retarded during expiration. And were these veins possessed of valves, a slight congestion would be produced at particular parts of their course during the act of inspiration, which might influence the functions of the cord, and prove very injurious in cases of difficult respiration. But being destitute of valves, the retardation produced by the act of expiration is diffused over the whole, and not limited to particular parts of their course. Hence, the congestion in these vessels must be general before its effects can be greatly manifested, and not confined to one particular part of the spine, as the symptoms of the disease often indicate.

And this circumstance of the pain being often confined to one particular part of the vertebral column, and greatly increased by pressure, would indicate that the disease is of a local character, and seated in that portion of the spine. If we examine the spine in these cases, there is not much pain complained of on pressure being employed along each side of the vertebral column, which we should have expected if the disease had depended on congestion of the cord. But if we press the intervertebral spaces, in the centre of the spine at the part affected, acute pain is felt, the patient winces, or syncope may even be produced. And if we move the fingers laterally across the part, a sort of crepitation often seems to be felt, as if a slight effusion were present,—a fact which we believe has not previously been mentioned.

From these considerations we conclude that the disease depends on inflammatory action of a low specific character, affecting a part of the spine and its ligaments, and gradually involving the membranes of the cord. The disease often appears to commence at a very small spot of the spine, and may remain confined to this spot for a considerable time without giving rise to much uneasiness to the patient; and the symptoms may even disappear if the constitution regain strength and vigour. But if the disease proceeds, more serious effects ensue. The part of the spine affected becomes acutely painful on pressure; and then the functions of the corresponding portion of the cord become impaired, producing, according to the particular seat of the disease, diminished power of respiration or locomotion, &c. In short, as we shall afterwards see, the complaint may terminate in decided and permanent affection of the spine.

Another circumstance to which we must advert in connection with the pathology of this disease is, that these affections have been most frequently observed in females, and generally considered as peculiar to that sex, and connected with irregular menstruation. But we are of opinion that this is not the case; for many patients have come under our observation and treatment, labouring under this affection, who were perfectly regular in the uterine secretion during the whole period of treatment, and had continued so before any symptoms of the complaint appeared. And the constitution may suffer from this disease at an early period of life, even before the age of menstruation, so that its non-appearance cannot in these instances be considered the cause. We have seen these spinal affections in young females before the age of twelve, who had been seized with the complaint, and suffered much from it for a considerable period, and who perfectly recovered before the appearance of the menses. And after the cessation of the catamenia, females who are free from every uterine disease may labour under these affections. We have witnessed cases of this nature distinctly manifested after the age of fifty, and the symptoms often prove very tedious and difficult to remove. Hence it appears that the state of the menstrual function is not to be considered the cause of these affections or the ground of their development. Cases, no doubt, often occur where we find a patient labouring under what is termed Spinal Irritation, at the same time irregular in the uterine secretions; and the circumstance always occasions anxiety to herself and her friends, as they believe that on this much of her affection depends. But if we can place any reliance on observation, it is to be considered only as coincident, and not as connected with the development of the disease. We must in medicine, as in physical science, endeavour to trace every effect to its true and specific cause, in order to establish general principles. For we may rest assured that, however varied may be the phases which disease may assume, there is nothing anomalous in its character, but that it all depends on general laws for the development of its symptoms. And accordingly, in these different cases, we conceive that one general law operates in producing the affection.

It is further to be observed that these affections are not confined to females. They are also observed in men; at least symptoms perfectly similar, and yielding to the same mode of treatment, are found to affect males; and it is very probable that they arise in a similar state of constitution. The patients, of both sexes, in whom these affections are most frequently observed, are those in whom there is an apparent want of tone and vigour in the constitution; where the functions of nutrition are languid, and there is indication of what is termed a strumous diathesis. Hence, attention has been directed to the state of the general health, and means employed to strengthen the system, and improve the digestive organs.

These complaints, we may also observe, have been considered of a hysterical character, and treated on that principle; and the varied symptoms which they assume might have led to that conclusion, whilst pathology afforded nothing definite respecting their nature. But, by a more correct diagnosis, the spine has been pointed out as the seat of the affection. And perhaps it may be asked whether or not hysteria has its seat in the spinal cord, or is in some measure connected with the functions of that organ. At all events, since the subject of spinal irritation occupied the attention of the profession, the number of hysterical complaints has diminished, and, there is little doubt, will be still further reduced as we obtain more correct views of the nature and treatment of disease.

But as the nature of these affections will be better understood from the perusal of the cases themselves, we shall describe a few that have come under our observation and treatment.

Case 1.—M. P., aged twelve years. This young lady has been for upwards of two years in a delicate state of health, and gradually lost flesh and strength. She is so weak that she is scarcely able to stand erect, or walk without assistance; in consequence of which she is principally confined to the sofa. She complains of pain in the chest and abdomen, and weakness of the back, particularly felt when she walks or stoops. She has much pain and weakness of the limbs; the joints, as she describes it, bend under her, as if unable to support the weight of her person; and her legs frequently start up when in a quiescent state. She has also some difficulty in micturition. She is liable to severe headaches, and occasional giddiness. The appetite remains good; pulse 80; the bowels are constipated. For these complaints she has taken much medicine; but in the treatment of the disease no attention has been directed to the spine.

On examining the spine, tenderness is felt on pressure over all the dorsal region of the spine, but more particularly at the lower portion of it, the pain being most acute at the intervertebral spaces.

Treatment.—Blisters were repeatedly applied along the spine, and the patient took purgative medicines; and in three weeks she felt much better; the pains of the chest and limbs had diminished, and she had more power in walking. By the continuance of these remedies, accompanied with small doses of tonics, in about two months from the commencement of the treatment, she completely recovered, and was restored to the enjoyment of good health.

Case 2.—M. A., a young woman, aged 21 years. This patient has been long in a delicate state of health. Her complaints, she says, commenced with attacks of sick headach, indigestion, and vomiting of food, to which she is still frequently subject. She has severe pains in the chest, the integuments of which are often so tender to the touch as scarcely to admit the pressure of her clothes on it. She is much affected with shortness of breath and palpitation of the heart on exertion, which renders it distressing for her to walk quick, or to ascend an eminence; and she is easily thrown into a state of cold perspiration. She complains of tightness across the epigastrium, uneasy sensations and distension of the stomach after taking food, from which she is often not relieved till vomiting takes place. She has severe pains in the abdomen, with difficulty of micturition, and even in evacuation of the bowels. Pressure on the abdomen aggravates her sufferings. She states that she feels pain and weakness in the spine; weakness and trembling of the limbs in walking; and aching of the joints after exercise. The pulse is 86; the tongue furred; the bowels constipated; the catamenia are stated to be regular.

As this person has been long an invalid, she has received much medical treatment for the removal of these complaints. She has had leeches repeatedly applied over the stomach, and blisters have been applied to arrest the vomiting, and remove the pain in the bowels. Blisters have also been applied to the chest; and she has taken much medicine internally. Attention has not been directed to the spine.

No indication of disease can be detected in the chest after the most careful examination. There is pain on pressing the abdomen, but not that acute tenderness which is observed in peritonitis, and its other symptoms are absent.

On examining the spine, tenderness is felt over a great portion of the dorsal vertebræ, and also to a considerable extent in the lumbar region. Pressure in some of the intervertebral spaces causes the patient to wince and start up.

Treatment.—Blisters were applied several times along the spine; she took preparations of iron, &c., and occasionally purgatives, containing small doses of croton oil, to keep the bowels free. Exercise was interdicted, and the patient was ordered to preserve the horizontal posture as much as possible. Under this treatment she improved. The vomiting ceased; her appetite increased; and she had greater power and vigour in the movement of her limbs. But the pain and weakness of the spine not being removed, and some of the other symptoms still continuing, close to the spine were established two issues, which produced decided relief. The pains diminished; she gradually regained strength; and in the course of a few months was restored to better health than she had enjoyed for years.

Case 3.—J. D., a young gentleman, aged 26 years, complains much of giddiness and pain of the head, with severe pains and weakness of the limbs, being easily fatigued, and oppressed with slight exertion, and has greater weakness in the left leg than the right. He is frequently attacked with severe pain and spasm referrent to the bowels, with tightness of the chest, and difficulty of breathing; and has generally much irritability of stomach, with indigestion of food. These complaints he had felt more or less for the period of two years; but of late they have increased to such an extent as to render him unable to undertake any considerable exertion without much suffering, or being afterwards confined to his room for several days. The pulse is 82; the tongue furred; the bowels are constipated. He has felt, he says, little or no pain in the back; but on his attention being directed to it, states that it does feel painful on stooping, and that he has a little difficulty in micturition. The remedies employed have principally been directed to the state of his stomach and bowels, and purgatives have always afforded temporary relief.

On examination of the spine, acute pain and tenderness are felt on pressure being applied to several portions of the dorsal and lumbar vertebræ; the pressure on the intervertebral spaces causing him to wince, and start up involuntarily.

A potass issue was applied to the lower portion of the dorsal vertebræ, and another considerably higher up the spine, at the part where the pain was most severe on pressure; and he was ordered to remain as much as possible in a horizontal posture. Purgatives were exhibited to keep the bowels free, and iron was administered as a tonic; and, in the course of a few weeks, he expressed himself decidedly better. By the time that the issues began to heal,—about eight weeks from the commencement of the treatment,—he had regained strength and vigour in his limbs; the pains and attacks like spasm in the bowels, with which he was so frequently seized, had disappeared; and he was so far improved in every other respect as to consider himself convalescent. This patient has continued to enjoy good health ever since; and since that period three years have elapsed.

Case 4.—Mrs W., a married woman, aged 38 years, who has had several children. This person complains of great and increasing debility,—pains of the chest, with shortness of breath, and palpitation of the heart on exertion. She has frequently severe pain and distension of the stomach after food, pain in the bowels, and difficulty in micturition. Sharp pains frequently dart down the thighs, and she feels great weakness of the limbs in walking, being fatigued and thrown into a state of cold perspiration by very slight exertion. Pulse 80; catamenia irregular; bowels constipated. These complaints, she states, have been of more than two years' duration, and generally been considered of a hysterical nature. She has been repeatedly leeched and blistered over the stomach, and had much medicine exhibited for her relief; having been frequently under medical treatment during the period of her illness. The complaint has not been considered as connected with the spine.

On examination of the spine, tenderness is complained of over all the dorsal region; the patient wincing under the pressure, and stating that it affects her much.

By the repeated application of blisters to the spine, in conjunction with the exhibition of strong purgatives, she improved and gained strength; the pains of the chest and limbs, and other disagreeable symptoms, abated; and in the course of a few weeks she considered herself well, and restored to health. On convalescence, the catamenia became regular. She has continued in good health, and nearly three years have elapsed since that period.

Case 5.—F. N., a young gentleman, aged 28 years. This patient has felt his health declining for nearly three years. He is less capable of enduring fatigue than formerly; perspires easily, and is very languid after exertion. At the commencement of these complaints he was affected with headaches, giddiness, and slight dimness of vision, which frequently attacked him suddenly when walking, rendering him liable to stumble, or almost to fall. Shortly after this he was seized with difficulty in breathing, which sometimes amounted to great oppression, as if he had not power to open his chest sufficiently. These symptoms still continue, and are particularly felt on ascending an incline, when he is often obliged to stand to take a full inspiration, and, at the same time, sharp pains shoot across his chest towards the sternum. The appetite is impaired, accompanied with indigestion, nausea, pain of stomach, and occasional vomiting of food. He often feels considerable uneasiness in his back after walking or much exercise, which causes him to assume the horizontal posture, from which he always obtains relief. He has frequently pain in his bowels, and a slight difficulty in micturition, pains in his loins, and darting down his thighs, and is feeble in the movement of his limbs. The left leg is weaker than the right, and they are liable to start up when at rest. Pulse 84; tongue slightly furred; bowels constipated.

There is no indication of disease in the chest or abdomen. On employing pressure along the spine, pain is complained of at the upper part of the dorsal vertebræ, and also between the first and second lumbar vertebræ. At these parts issues were applied, and he was ordered to retain the horizontal posture, and to take purgatives. In the course of two weeks his symptoms began to improve. He could respire with more freedom; the nausea and pain of stomach had abated, and he had greater power in the movement of his limbs. We now employed gentle tonics, and still continued the use of the purgatives; and, in the course of a few weeks, renewed the issues, which had begun to heal. In this manner he continued to improve; and, in about ten weeks from the first commencement of the treatment, he considered himself convalescent. He still, however, continued the occasional use of the purgatives, which had always afforded him relief; and, in a short time, he felt himself strong, and restored to good health.

Case 6.—J. S., aged 33. This person has, for a considerable time, been in a weak and delicate state of health. He complains much of pain and uneasiness in the lower part of the back, with difficulty in passing his urine. He feels great inconvenience from stooping, as the pain becomes severe on raising himself into the erect position. He is liable to headaches and giddiness, being sometimes apt to fall whilst walking. He states that he has less power than formerly in moving his limbs; that a cold sensation runs down the outer side of the left thigh; and that this limb is weaker than the right. Is easily fatigued by walking, and perspires much after exercise. His legs also often start up involuntarily when in bed, or remaining in a quiescent state. These complaints being considered of a rheumatic character, were treated on that principle, but with little or no abatement of the symptoms. Pulse 84; tongue furred in centre; appetite weak; bowels regular.

On examining the spine, acute pain is felt on pressure being applied to the intervertebral spaces of the lower portion of the lumbar vertebræ, and slight tenderness is also complained of at one spot of the dorsal vertebræ; but sensation does not appear to be impaired in the integuments of the limbs.

Potass issues were applied at that portion of the lumbar vertebræ where the pain on pressure was most acute. Powders, consisting of a preparation of iron, were exhibited internally; and he was recommended to retain the horizontal posture as much as possible, and to take a croton oil pill occasionally. In a few days he stated that he felt himself better than he had been for several months; that he had regained complete power over the function of the bladder; that the strength and energy of his limbs were improving; and in a few weeks he felt so well as to engage in a very active occupation.

Case 7.—J. P., a man aged 45 years. Has for these last twelve months felt considerable pain and weakness in the lower part of his back, during which period the state of his health has been much impaired, being easily fatigued, and liable to perspire on slight exertion. The complaint of his back has increased of late, rendering him unable to undergo much fatigue, as the pain is always worse with exercise, and easiest whilst he remains at rest in the horizontal posture. He has also a constant painful sensation in the lower part of the abdomen, immediately above the pubes, with difficulty in passing water,—a complaint which distresses him much. He states that he has less energy than formerly in moving his limbs, which is particularly evident in descending a declivity, when his knees, as he expresses it, nearly bend under him. Acute pains are felt extending down the thighs, and his legs occasionally start up when at rest. Pulse 80; tongue furred, and appetite weak.

On examining the cervical and dorsal portion of the spine, he does not complain of pain. But tenderness is felt on pressure at the intervertebral spaces of the lower portion of the lumbar vertebræ.

At the place now mentioned a potass issue was applied, and he was recommended to assume the horizontal posture, and to take tonics of columba, &c. In the course of a few days, he was decidedly improved in health, had greater power and freedom in passing his urine, and felt stronger and more vigorous in his limbs; and, in about two months, was so far restored to health, as to engage in the duties of an active occupation.

Case 8.—J. C., a young woman, aged 26. Nearly four years since, she felt her health beginning to decline, became weak, languid, and more easily fatigued than usual. Shortly after the commencement of these complaints, she was attacked with headaches and giddiness, which affected her walking, rendering her sometimes liable to fall, or to seize hold of an object to support herself. She had also acute pains in the arms, with a sort of prickling sensation in the fingers. These complaints have continued more or less since the commencement of her illness, without being much benefited by the remedies employed for their removal. At present she suffers greatly from irritability of stomach, nausea, and vomiting of food. Is liable to giddiness and dimness of vision; and has often severe pain across the lower part of the abdomen, which amounts almost to tormina, if she has taken anything difficult of digestion. There is often an uneasy sensation in the lower parts of the spine; and she complains of pain and weakness of the limbs in walking. She seems to walk with difficulty across the room. Pulse 82; tongue furred; bowels constipated. Catamenia have always been regular.

For these complaints she has taken much medicine; has been blistered on the epigastrium, and at the part of the chest where the pain was most severe. But no attention has been given to the spine.

On examining the spine, acute pain, and tenderness on pressing, are manifested along its whole course, but more particularly about the lower portion of the dorsal vertebræ, where pressure in the intervertebral spaces causes her to shriek.

Counter-irritation was applied to the lower portion of the dorsal vertebræ, where the pain was most severe on pressure. She took purgatives, and small doses of preparations of iron, and was ordered to retain the horizontal posture. Shortly after the application of the counter-irritation, she considered herself worse; the pains of the abdomen and limbs increased, &c. But in the course of several days the symptoms began to improve, and in a few weeks she was decidedly better, and had more power and energy in the movement of her limbs. Counter-irritation was then applied along the upper portion of the spine, and the other remedies were continued, and succeeded by still greater relief. The nausea and vomiting abated; the pains of the chest and weakness of the arms were removed; her strength increased; and in a few months she expressed herself well and in good health.

Case 9.—A young girl, aged 10 years. This patient one evening fell down suddenly in a state of insensibility, from which she could not be aroused for some time. On coming out of this state, she appeared wild and delirious, talked incoherently, and was unable to give a proper description of her feelings. She seemed weak and anæmic, with a quick excitable pulse, and furred tongue; and her parents stated that she had felt delicate for a considerable period. On examining the spine, much sensibility was evinced on pressure being applied to the upper part of the dorsal vertebræ. She shrieked, and complained of pain. A blister was applied to this part of the spine, and a dose of purgative medicine exhibited; and next day she was much improved, calm, and collected. She stated that she had of late been liable to giddiness in walking, that she felt acute pains in her side and chest, and weakness in her limbs, that her appetite was bad, and she perspired on very slight exertion. Preparations of iron were exhibited, and her bowels kept free by aperient medicine; and in a few days she was perfectly well.

This is a case in which the symptoms might very readily have been referred to the brain, and treated on active principles. And it is very probable, had this mode of treatment been adopted, it would have proved most injurious to the patient. But attention being directed to the spine, the seat of the disease, and appropriate remedies employed, she speedily recovered.

Case 10.—Mrs R., aged 28, married, and has had one child. About two years since she fell into a delicate state of health; she became weak, and easily fatigued; was affected with headaches and giddiness; pains in the side, and palpitation of the heart on exertion. Her appetite was bad, and she suffered much from indigestion and vomiting of food. She had severe pain extending along the margin of the ribs, on the right side of the chest, increased by walking, stooping, and every kind of exercise, which often became so acute as to prevent the pressure of her clothes on it. The complexion also assumed a dingy yellowish hue, like that of a person whose biliary secretions were deranged, which led to the belief that her liver was affected.

She states, that of late she is sensible of increasing weakness in her arms, which have become affected with a sort of prickling sensation in her fingers, that extends to the elbows, and gradually to the shoulders, and that then the pain becomes particularly severe. She is also seized with sharp shooting pains in the mammæ, in the chest and sides, and extending across the lower part of the abdomen, and has considerable difficulty in micturition. She does not complain of much pain or uneasiness in the spine, but of weakness in her limbs, and severe aching pains in them after exercise. Pulse 78; tongue white and furred in the centre; bowels constipated; catamenia regular. In the treatment of this complaint no attention had been directed to the spine.

On examining the spine, she complains of tenderness on pressure in several parts of it, but more particularly at the upper portion of the dorsal vertebræ. To this counter-irritation was applied, and she was at the same time ordered to take croton pills as a purgative, and small doses of preparations of iron. Shortly after the application of the counter-irritation to the spine she felt worse; the pains of the arms, the chest, and side, increased; and she had more frequent attacks of vomiting of food. But in a few days these symptoms abated, and then the counter-irritation was applied to the other parts of the spine, where the pain was most acute on pressure, and the other remedies continued; and in the course of two months she considered herself well, and restored to excellent health. In this case, signal benefit seemed to be derived from the exhibition of the croton oil pills.

The consideration of the cases now adduced demands the following observations.

1st, From these cases we perceive the variety of symptoms which affections of this class assume, and how liable they are to be mistaken for other complaints, and treated on erroneous principles The pains are felt at different parts of the body, and general lassitude, or want of power in a particular limb, is complained of, but no cognisance is taken of the spine, the seat of the disease. And it is often only after the complaint has existed for some time, that the patient becomes sensible of weakness in that part of the body, and the attention of the physician is directed to it. Hence it is of the utmost importance, in those cases where positive disease cannot be detected in the viscera, which appear to be the seat of severe and protracted pain, to examine the spine, and see if tenderness be not recognized on slight pressure of a particular part. We have thus seen symptoms which had resisted much treatment, speedily disappear, and patients restored to the enjoyment of excellent health, after great debility and suffering.

2d, The disease depends on inflammatory action of a low specific character, affecting a particular portion of the spine, as the symptoms evidently indicate. The painful feeling is increased by external pressure, which is most distinctly manifested at the intervertebral spaces; a very slight pressure with the point of the finger on this spot often causes the patient to shriek from the severity of the pain. It is also aggravated by walking, stooping, and every kind of exercise that increases the motion of the spine, and relieved by rest and the horizontal posture,—which cannot be accounted for by congestion of the cord; and if we move the fingers across the part, a sort of fulness or puffiness often may be felt, as if a slight effusion were present,—a symptom which is not recognized in a healthy portion of the spine. In short, the acute sensibility to pressure manifested at a particular part of the spine, in accordance with the other phenomena, clearly indicate that the disease must be considered of an inflammatory character, and not dependent on congestion.

Treatment.—This affection not being dependent on what is termed acute inflammatory action, but on inflammation of a subacute, or specific character, occurring in patients where there is a want of tone and vigour in the constitution, does not admit of active antiphlogistic treatment, and general bleeding is not well sustained. We have seen cases where it had been repeatedly employed, but with no benefit to the patient, but an aggravation of the symptoms,—being followed by greater weakness, and an increase of pain and irritability. Even local bleeding, as cupping and the application of leeches, which may be employed with greater freedom, does not seem so productive of benefit as counter-irritation to the spine. The treatment which we consider the most beneficial, is the frequent employment of purgatives, and the croton oil pills will be found of essential service, exhibiting, at the same time, preparations of iron as a tonic, accompanied with rest in the horizontal posture, and counter-irritation to the spine. And, in reference to the last remedy, we may observe, that blisters do not appear so efficacious as a strong rubefacient, or ointment of tartarized antimony, rubbed along the vertebræ, till a copious eruption appears at the part where the pain and tenderness on pressure is complained of. But when the affection is more severe and of long standing, a few potass issues applied along the spine are productive of the greatest benefit. These are the remedies deemed most efficacious in the treatment of this affection,—from the employment of which we have most frequently witnessed an improvement in the symptoms, and ultimately the restoration of the patient to health.

(*To be continued.*)

Wetherby, Yorkshire, 5th January 1850.

ART. III.—*On the Effects of* RHUS VENENATA *or Poison-Sumach; with a Case.* By THOMAS STRATTON, M.D.; Surgeon, Royal Navy.

IN Rees' Cyclopædia, and in the London Encyclopædia, reference is made to Kalm, Lyon, Pursch, and Martyn, who have observed various effects from some of the species of *Rhus;* from their accounts, it appears that some of the species produce symptoms different in degree and somewhat in kind in different persons, while in some individuals they produce no effect.

In 1843, I was stationed at Penetanguishene, on Lake Huron, at which place there are several species of *Rhus* not uncommon, and the residents are acquainted with the occasional consequences of handling some of these.

On the 22d of November, I was requested to visit Antoine Lafreniere, a French-Canadian farmer, living two miles west from Penetanguishène; and while driving to his house, the messenger casually told me that his son had been "poisoned" by a plant the day before. He gave me this information as a thing of no moment; and, on reaching the house, and examining into the state of the father, who had an attack of acute rheumatism, I inquired for his son, whom I found in bed.

Olivier L., aged 16, a short, stout lad, in the afternoon of the 21st of November, while ploughing in a field, came to some shrubs in the way of the plough. These he removed with his hands, on which there previously were some abrasions. In about an hour afterwards, his hands, arms, face, and legs, began to swell; this was accompanied by slight redness of the skin and by thirst; no nausea and headach.

Nov. 22d, Great swelling of the arms, face, and legs. Some slight swelling and redness over the chest; but the trunk of the body generally is not much affected. Pulse and tongue natural. I sent some of the people in the house out to the field for the plant, which was the *Rhus Venenata*, or Poison-Sumach (Eaton and Wright's *North American Botany*, 1840, *Albany*, page 392).

The swelling continued to increase till the evening, taking about twenty-eight hours to reach its height.

Nov. 23*d*, He is still in bed. The swelling and redness are stationary.

Nov. 24*th*, The swelling begins to lessen, and the redness has nearly disappeared.

Nov. 26*th*, This morning he left his bed. The swelling and redness have disappeared.

Nov. 27*th*, The swelling took about twenty-eight hours to reach its height,—was about ten hours stationary, and two days declining, the attack having lasted altogether four days, during which time his appetite was the same as in health, and he kept his bed only on account of the swelling, which rendered him unable to bend his knees, or to move his legs. No external application was used, and no internal remedy was taken, except laxative medicine. It was the first time he had been attacked in this way. As at the time I visited the house daily to see his father, I had an opportunity of observing the course of Olivier's case, for which the French-Canadians here would not consider it necessary to apply to a medical man. The popular ideas regarding this complaint are, that it is not at all dangerous; that it lasts about nine days; and that frictions with sour milk are beneficial. Many of the lower French-Canadians are very ignorant and credulous, and entertain certain strange notions respecting this plant. I heard one say that he knew persons who began to swell immediately on seeing it, and others who did the same on hearing its name (*l'arbre à puce*) mentioned. In 1844, a gentleman travelling in this part of Canada was very inconveniently delayed for a week by an attack of swelling from touching the plant, while two other persons staying at the same house rubbed (as an experiment) some of the leaves over their faces and hands, without any result being produced. I understand that the Indians are occasionally affected by it in the same way as their White brethren.

In May 1844, after gathering some of the plant, my right hand swelled very much without redness, itching, or other symptom: this increased for about twenty-four hours, then declined for about the same time; and on the third day the swelling was gone. There was no desquamation. The left hand was not affected. After a few days I rubbed these leaves over my hands without any effect.

In this complaint the appearance of the swelling is very sudden, and its increase very rapid; and when situated in the face and scalp, it produces considerable discomfort. As this would likely create anxiety in the minds of those who did not suspect its cause, I hope that, to travellers in those parts, this short notice may be of service in preventing unnecessary alarm.

Royal Marine Infirmary,
Chatham, October 1849.

ART. IV.—*Contributions to the Statistics of Valvular Disease of the Heart, with special reference to Affections of the several Orifices, and the particular effects of certain recognised causes.*

By A. WHYTE BARCLAY, M.D., Physician to the Chelsea, Belgrave, and Brompton Dispensary, and Med. Registrar to St George's Hospital. [Medico-Chirurgical Transactions, Vol. xxxi. London, 1848. P. 185.]

THE whole number of deaths taking place at St George's Hospital during two years previous to 14th April 1848, was five hundred and thirty-five (535). The condition of the organs of circulation was ascertained in four hundred and nineteen (419) instances; and among these were seventy-nine cases of valvular lesion.

Fibrinous deposit, by which is meant that condition of the endocardium usually ascribed to acute inflammation, without adopting the hypothesis which attributes it to deposition from the blood, in preference to exudation from the membrane, occurs sixteen times among the first eighteen cases, reaching to the age of thirty-four; and is mentioned only five times among the remaining fifty-one cases. In fourteen instances previous valvular disease existed; while in seven the fibrinous deposit on the valve is the only condition of the disease.

The valves affected in the seventy-nine cases given in the tables are,—

In thirty-six cases (36), the aortic and mitral valves simultaneously; or 45 per cent.

In twenty-six cases (26), the aortic valves alone; or, 33 per cent.

In seventeen cases (17), the mitral valves alone; or, 21 per cent.

These numbers, the author thinks, may be taken to represent pretty correctly the relative proportions of valvular disease in general; for, on a survey, extending to nearly 200 cases, he finds the numbers to be,—

For the aortic and mitral valves together,	46	per cent.
For the aortic valves alone, ...	32	...
For the mitral valves alone, ...	22	...

Taking for standard of comparison the whole number of inspections in which the condition of the heart is reported, namely, four hundred and nineteen (419); the results are per cent.—

Of valvular disease, ...	18·8	per cent.
Of the aortic and mitral valves simultaneously,	8·6	...
Of the aortic alone, ...	6·2	...
Of the mitral alone, ...	4·0	...
	18·8	

The arrangement, therefore, is, that disease of the aortic and mitral valves is most common; that next in frequency is disease of the aortic valves alone; and that least frequent is disease of the mitral valves alone.

Among the 419 deaths from all causes in the hospital, one hundred and thirteen individuals (113), about twenty-seven per cent., are reported to have presented traces, more or less marked, of atheromatous disease, either in the valves themselves, or in their immediate vicinity, at the origin of the aorta. The researches of pathologists have sufficiently proved this deposit to be a disease chiefly of advanced life; and the following list of the numbers at which it appeared at different ages is given in the way of illustration.

1	Case at the age of............	6
1	... ...	12
1	... ...	19
13	Cases between the ages of...	20 and 30
22	... ...	30 and 40
19	... ...	40 and 50
36	... ...	50 and 60
13	... ...	60 and 70
7	... above the age of....	70

Comparing these numbers with the proportion of deaths at corresponding ages during the same period, it results that, among all the patients dying in hospital, the proportion exhibiting traces of atheromatous disease in the immediate proximity of the heart was at different ages as follows:—

From 20 to 30	12 per cent.
From 30 to 40, and from 40 to 50, each	21 ...
From 50 to 60, and from 60 to 70, each	43 ...
And above 70	54 ...

The deposit is nearly twofold more frequent between 30 and 40, and between 40 and 50, than between 20 and 30; more than twofold more frequent between 50 and 60, and 60 and 70, than between 30 and 50; and above 70 it increases at the rate of about one-fourth.

In about two-fifths of the cases of atheroma, valvular disease is superadded; but in little more than one-half of these was the valve found in a state of atheromatous degeneration.

Among the cases recorded in the table are twenty-eight instances of atheromatous disease affecting valves; fourteen of which are of atheroma at the root of the aorta; and twenty-four instances of atheroma of the aorta, in which the valvular affection was of some other nature. This gives a total amount of fifty-two instances of atheroma among seventy-nine instances of valvular disease; and, as already stated, 113 altogether for two years.

Taking the 419 inspections, as the standard of comparison, the following relative proportions are the result:—

27 per cent. of atheroma on the valves, or in their immediate vicinity.

12·24 per cent. of atheroma, conjoined with valvular disease.

3·34 per cent of atheroma of the valves, with similar condition of the aorta.

·73 per cent. of atheroma of the aorta, with some other form of disease of the valves—

3·34 per cent. of atheroma of the valves, the aorta being healthy.

The twenty-eight cases of atheromatous valves consist of—

4 affecting equally the aortic and mitral valves.

12 affecting the aortic valves.

of which were	1	associated with fibrinous deposit on the mitral valve.
	6	... with thickening of the mitral valve.
	5	... with no form of mitral disease.

12 affecting the mitral valve—

of which were	1	associated with fibrinous deposit on the aortic valves.
	2	... with thickening of the aortic valves.
	9	... with no form of aortic disease.

In two of these cases, fibrinous deposit on the one valve corresponds to an atheromatous condition of the other. This conjunction of long-continued atheromatous disease with recent inflammatory action is not rare. Eight examples of the conjunction are recorded. Besides the two instances now mentioned, one case presents atheromatous and fibrinous deposits on both sets of valves; another atheroma of the aortic valves, accompanied by fibrinous deposit on the aorta itself. In a third instance, atheromatous and fibrinous deposits on the aortic valves are associated with thickening of the mitral valves; in a fourth case, the same combination of disease in the mitral valve is conjoined with thickening of the aortic valves; and in two others it is found without other cardiac lesion.

In several of these cases other acknowledged causes of cardiac inflammation had been present, and might alone, or in concurrence with the atheromatous condition, tend to induce the inflammatory action, the traces of which are found after death. In one instance, recent pleurisy; in another, pericarditis; in a third, granular degeneration of the kidneys; and, in two other instances, inflammation in other organs,—show that we ought to be cautious in assigning great importance to the previous atheromatous and contracted state of the valve. In other instances no cause can be assigned. The author refers to Nos. 7 and 8 as instances of this category, and furnishing reasonable grounds to infer that the inflammatory condition was in great measure induced by the previous condition of the valve, which was such as to offer great impediment to the healthy action of the heart.

Attention is directed to another set of cases of opposite character, in which atheroma seems to have been developed in a valve previously thickened, contracted, or even cartilaginous. As this circumstance, however, is believed to be more properly considered after the cases commencing in inflammation, it is merely stated as a fact, that the results of statistical analysis are opposed to the statement made by several pathologists, that the mitral valves are more liable to atheromatous disease than the aortic. The only difference in the cases examined in this table, is, that when the mitral is affected with atheroma, the aortic valves are more frequently found free from other forms of disease than the contrary.

In only fifteen cases was a distinct account obtained from the patient of his having undergone at some previous period an attack of acute rheumatism. In many other cases it was ascertained that he had never suffered by acute rheumatism; in some the fact was doubtful; and in others it had never been inquired into. It may be supposed that in many cases, in which merely slight thickening or opacity of the valve was found, giving no evidence of their unsoundness during life, or where attention was directed to some other ailment, independent of the central organ of circulation, which ultimately proved fatal,—though the affection of the valve might be due to acute rheumatism, yet having been overlooked during life, no inquiry as to the cause had been made. This is particularly true as to patients dying in the surgical wards. Further, if the difficulty of obtaining from patients themselves a distinct account of any previous illness, or of the origin of their present ailments, be considered, it may be allowed to be probable, that of all the instances proceeding from this cause, those stated form only one portion.

This question becomes important, when it appears that the fifteen cases consist of—

13 of affections of the aortic and mitral valves together.
1 ... the aortic valves alone.
1 ... the mitral valves alone.

This affords a strong contrast to the results of the statistics of valvular disease in general, already given. But it is remarkable that, in tracing these cases backwards over two or three years previous, a similar result is obtained, and that, among thirty-five cases ascribed to the effects of rheumatic endocarditis, there are—

28 of affections of the aortic and mitral valves together.
3 ... the aortic valves alone.
4 ... the mitral valves alone.

To one possible source of fallacy it is proper to advert. It must be evident, that the majority of those persons to whom the question may have been put, were patients in whom the affection must have been so well marked as to leave no doubt that the valves were unsound during life; and, therefore, many or most of them were cases which ultimately ended in death, chiefly in consequence of conditions arising out of that unsoundness. But in other cases fatal in consequence of similar conditions, likewise traceable to disease of the heart, in which acute rheumatism had not existed, the same proportion does not hold. Of the fifteen rheumatic cases, nine proved fatal from disease directly connected with unsoundness of the heart; of which there were—

8 instances of disease in both aortic and mitral valves.
1 instance ... in the aortic valve alone.

In the other six there were diseased kidney and albuminuria.

During the same period are recorded twenty-six cases in which the cardiac disease was equally well marked during life; and nine of these were connected with albuminuria. The remaining seventeen consisted of—

7 cases of disease of the aortic and mitral valves simultaneouly.
4 ... ... the aortic valves alone.
6 ... ... the mitral valves alone.

Dr Barclay, consequently, no doubt, entertains that instances of cardiac lesion, proceeding from rheumatic *endocarditis*, are peculiarly liable to simultaneous affections of both sets of valves.

Almost all cases of disease of heart in connection with rheumatic fever prove fatal at a comparatively early period of life. Ten of the first seventeen cases are stated to have had attacks of it, and to have died at ages from twelve to thirty-one. In only one of these instances was the rheumatic attack recent. In another case a recent attack supervened on one of long standing, and no new inflammation was excited in the pericardium; and in a third case, purulent inflammation of the joints was present without endocarditis, and a history of an attack of rheumatism some years previously was given. In the five cases occurring at later periods of life, the patients, in like manner, refer to attacks undergone some years previous to the period of their death; and in one only was there severe lesion of the valves of some duration, (No. 24). In three others the pericardium was adherent; and one case (No. 37) Dr Barclay considers as mere coincidence, since the rheumatic fever had taken place sixteen years previously; the pericardium was not adherent; one of the valves was atheromatous, but the valvular lesion was no where of great extent; while the kidneys presented the granular degeneration.

Acute rheumatism is essentially a disease of early life. During the two past years at St George's Hospital, one patient at fifty-five, and one at fifty years, were admitted, laboured under an acute form of the disease.

			Patients.	
Between 15 and 25	...	...	71	were admitted.
Between 25 and 35	...	...	50	...
Between 35 and 50	...	...	22	...
Below 15	...	...	7	...

Of these seven the youngest was ten years of age. This is allowed to furnish only approximate results, as it is known that children are liable to rheumatic attacks at a much earlier period; and there is reason to believe that the number under fifteen may be greater, but that young persons under that age are not frequently brought to the hospital. Dr Barclay directs attention to another circumstance, that, while during two years 152 patients labouring under this disease were in the hospital, excluding the subacute, gonorrhœal, and chronic forms, in only fifteen instances were traces of endocardial affection, in which a history of acute rheumatism was obtained, found on inspection during the same period. If to these be added all the doubtful cases in which the origin was not known, and the chief lesion was not atheromatous in kind, the number amounts only to thirty-eight altogether, in fourteen of which the affection was complicated with disease of the kidney.

A previous attack of acute rheumatism, complicated with cardiac inflammation, appears to confer a predisposition to suffer from subsequent endocarditis, although the rheumatic disease do not again manifest its presence in the joints. Eight of the first ten rheumatic cases present evidence of recent endocarditis. In one of these, as already mentioned, there had been a recent attack of acute rheumatism. In another, the patient was labouring under a severe form of pneumonia, terminating in disintegration of the structure of the lungs. In a third was found granular degeneration of the kidney. But in the other five, the chief ailment during life, and the principal cause of death, was referable to the state of the heart. Three were complicated with dropsy. Of these, two without dropsy, had recent pericarditis; three with dropsy had,—one pericarditis and pleurisy, one pleurisy, and one pneumonia.

Further confirmation of this inference, as to the predisposition thus bestowed, may be derived from four cases of fibrinous deposit found on other portions of the endocardiac membrane, all of which took place in instances of former rheumatic fever.

In nine of the fifteen rheumatic cases endocarditis was present at the time of the death of the patient. The majority of these gave evidence of the existence of previous disease. In two there was atheromatous deposit in an advanced stage. In the other six cases, the valves were, in two atheromatous, and in the remainder indurated and contracted. In all the instances in which fibrinous deposit was present, both valves were affected by some form of disease; the two examples of rheumatic affections of solitary valves being found among those only of long standing.

As to alterations taking place in the muscular structure of the heart, hypertrophy, dilatation, and atrophy, are mentioned in equal proportions nearly as consequences of severe lesion in the aortic and in the mitral valves, and of adherent pericardium; and in some instances they are found in a considerable degree of development, with a condition of valves in no way proportionate to the muscular changes.

It forms no part of the plan of the author to investigate generally the subject of the etiological relations of *pericarditis*. Of its relation, nevertheless, to valvular disease, some idea may be formed from the following facts.

Among the cases of valvular disease are fifteen instances of recent pericarditis. In seven of these the patient had at some previous period suffered from rheumatic fever; in six of the remaining eight this had certainly never been the case; in one instance it is uncertain; and in one it is unknown. Nine of the fifteen cases are associated with fibrinous deposit on the valve, and one with similar deposit on the lining membrane of the aorta, without its being found on the valves. There were also nine cases of old adhesions of the pericardium, six of rheumatic origin, two non-rheumatic, one of unknown origin.

The uncertainty involving the white patches often found on the surface of the heart, the difficulty of enumerating every instance, or wholly excluding them as evidences of inflammation, and the dependent question, how far all trace of pericarditis may be removed, are believed to justify the course adopted in the present report. Several of the instances, in which pericardial was combined with endocardial inflammation, furnished evidence, from examination during life, that the affection of the pericardium was subsequent to, and consequent upon, the inflammation within the heart; while in some a general cause seemed to operate in inducing both simultaneously, and each independently of the other; and in others, the inflammation being propagated from the neighbouring parts, may be presumed to have extended from without, inwards, and endocarditis to have supervened upon pericarditis.

In seven of the fifteen instances of recent pericarditis, and in five of the nine of old standing, these lesions are associated with similar conditions of the pleura. In seven instances also, recent pleurisy is associated with fibrinous deposit on the valves. In four of these, pericarditis was also present, and in three, the pericardium was unaffected. Numerous other complications were present, and in three only were the symptoms referable to the lungs and pleura in any way prominent, so that it is impossible to trace between pleurisy and endocarditis any association so definite as clinical observation led the author to anticipate. Several instances are recorded of old pleuritic adhesions, in which was found slight thickening of the valves, from previous inflammation, without any other lesion sufficient to account for its origin; but in all, the affection of the valve was slight, and the instances in which it was observed occur among persons advanced in life. Pleuritic adhesions are mentioned in thirty-four instances of valvular disease, and are associated in twenty-one with disease of the kidney; in two with rheumatic fever. Of the remaining eleven cases, three were instances of atheromatous thickening of the valves, and eight of simple thickening.

Deviations from the normal and healthy state of the kidney are noticed in forty-one instances in the table. The disease is,

In 28 instances, granular degeneration.
... 5 ... the kidney is large and mottled.
... 1 ... ... mottled, small, and the cortical structure wasted.
... 3 ... congested.
... 2 ... coarse.
... 1 ... fatty.
... 1 ... one kidney had a yellow patch on the surface.

During the same period, granular degeneration was observed altogether in fifty-nine cases; in twenty-one instances, the kidney was large and mottled; and in eleven it was mottled and atrophied. In nine of the last number it was granular, and five of these were coincident with granular disease; so that the author places the other two in which the surface was not granular, in the same general class of atrophied kidneys, in opposition to those which in the diseased state are increased in size.

In four instances, kidneys affected by other forms of disease are stated to have contained cysts; but in seven instances of simple cysted kidneys occurring during the same period, the valves were unaffected. It is remarkable that, while of sixty-one cases of atrophied kidney, twenty-nine are coincident with valvular disease, only five cases in twenty-one among those in which the kidney was large and mottled, present examples of endocardial affections; very nearly fifty per cent. in the one condition, and scarcely twenty-five per cent. in the other. This result among so large a number of cases must be regarded as indicating something more than a mere accidental circumstance, and acquires increased importance from the doubts entertained by some pathologists as to the identity of these two forms of disease, which have been supposed to be only different stages of the same disorder.

Among the twenty-nine instances of atrophied kidney are five examples of recent endocarditis, of which,—

1 is connected with previous rheumatic fever.
2 ... with a previously thickened state of the valves.
2 ... with no other morbid state.

In three of these old adhesions of the pleura were found,—in one recent pleurisy, in another recent pericarditis were also present; but in all the presence of disease of the kidney had been recognised during the life of the patient by the albuminous state of the urine; and, from careful examination of the cases, Dr Barclay thinks himself justified in asserting that this was the chief cause of the lesions found after death. The reason for this inference is given in the fact, that although there is reason to believe that a mutilated or inefficient condition of a valve may excite endocardial inflammation, yet, in the cases under review, the affection was scarcely of such extent as to authorise such a belief, and was of small importance in comparison with the coagulable state of the urine. The two last instances at least are free from this ambiguity.

The remaining twenty-four instances include,—

2 cases connected with rheumatic fever.
10 ... ... with an atheromatous state of the valve.
1 ... ... with atheromatous dilatation of the aorta.
11 ... ... with no direct cause of disease of the heart, except pleuritic adhesions.

In three only of the ten instances in which the renal disease was complicated with an atheromatous state of valve, does that appear to have been unaccompanied by inflammatory thickening; in some the state of the valve was such as to afford clear evidence that inflammation and thickening had preceded the atheromatous deposit; while in others it was impossible to ascertain which was the primary lesion. In six of those cases of which the author is disposed to attribute the primary cause to disease of the kidney as predisponent, pleuritic adhesions were found. While, on the one hand, it is to be considered that endocardial inflammation may probably be excited by pleurisy, it is to be remembered on the other, that it is in many instances due to renal disease, and is liable to take place without any perceptible cause in the course of renal dropsies.

The five instances in which valvular disease was coincident with the large and mottled condition of the kidney, consist of,—

3 of acute endocarditis {
1 complicated with calcareous deposit.
1 ... with previous rheumatic fever and cellular inflammation.

of acute endocarditis { 1 complicated with severe pleuro-pneumonia.
1 of simple atheroma of the mitral valve.
1 of thickening and ulceration, connected with no other cause of cardiac disease.

Whatever influence may be assigned to the condition of the kidney as predisposing to cardiac disease, it is worthy of remark that, in each of the cases of recent endocarditis now enumerated, the complications are such as are seen associated with the same form of acute disease, when the kidneys were healthy, which cannot be said of all instances of granular degeneration. In the first case only (13) was disease of the kidney indicated during life; excessive hæmaturia, though of short duration, had taken place previous to admission; the calcareous deposit on the valve was of long standing; and there were old adhesions of the pleura and pericardium. In the other two (41, 55), severe and disorganising inflammatory diseases of the lungs and cellular tissue were present, and are noticed as causes of the fatal result. In these instances, indeed, was no symptom to call attention to the condition of the kidney. In one of them also a previous attack of acute rheumatism had taken place; and this, it has been seen, has a tendency to confer a predisposition to subsequent attacks of endocarditis; and from this cause probably the adhesion of the pericardium proceeded.

The first of the two cases without recent endocarditis (22) cannot in any manner be attributed to disease of the kidney. The other (66) is more doubtful. The patient in whom this took place had been in the hospital twelve months previously with common continued fever, from which he completely recovered; but, during the course of this disorder, a murmur was discovered accompanying the first sound of the heart over the aortic valves, without evidence of the hypertrophy, which was well marked on the second admission; and during the intervening period he alleged that he had suffered from gout; the urine was coagulable. The investigation into the relations of these cases is important; for, though the most direct evidence of the connection between endocarditis and particular forms of renal disease, must undoubtedly be derived from those instances in which the former affection is quite recent, it is nevertheless reasonable to expect that, if it be of frequent occurrence during their continuance, traces of its previous existence shall be left in thickening and contraction of the valves after death. In comparing these two forms of disease, it is found that, while contraction with thickening of the valve exists in numerous cases of granular degeneration, only one ambiguous instance is recorded as coincident with a large mottled kidney;—a result which agrees with the fact that endocarditis occurs in the former without complications, and is not found in the latter except under such conditions as would seem to develop it in healthy kidneys. It is proper at the same time to observe, that the table contains only three instances of endocarditis between the ages of forty-five and fifty-five, and that two of these are coincident with large mottled kidneys, and one with granular degeneration, whatever other cause may have been operating; while, on the other hand, there is scarcely a larger proportion of cases of valvular disease coincident with mottling degeneration, than is found among deaths from all causes in the hospital.

Of the cases in which evidence of recent endocarditis was found, three remain which can be traced to none of the causes hitherto specified. The first of these (1) derives importance from being the only case in the list in which a general condition of inflammatory disease is accompanied by inflammation of the endocardium, without the intervention of any other assignable cause; and from its thus justifying the inference already stated, that even in instances in which such complications are present, reference to the history of the case may induce us to exclude them as causes of the endocarditis found on inspection. A mark of doubt is set upon the statement, that the patient in this case never had suffered from rheumatic fever, solely because he was said to have complained of aching pains in the limbs one day or two before the accession of the acute symptoms, although none of the joints had ever been swollen or red.

The other two instances (3, 28) possess one common feature; namely, the peculiarity of having only two aortic semilunar valves. Two similar instances appear in the list, exhibiting other forms of valvular lesion; and Dr Barclay is not aware that, during the two years from which the cases are selected, any instances of this abnormal conformation occurred, unconnected with endocardial disease. In none of these was the mitral valve in any manner affected; none gave any history of previous rheumatic fever; in none was any form of renal disease present; and in all, while two of the valves were unusually developed, the third was either quite rudimentary or entirely absent. To the congenital malformation the author ascribes the subsequent development of cardiac disease.

The four instances consist of,—

2 instances of recent endocarditis.
1 ...... of inflammatory thickening with ulceration.
1 ...... of calcareous deposit.

The first instance took place in a boy of fourteen years of age, who was admitted into the surgical ward with diffuse cellular inflammation, commencing at the ankles, extending up the legs to near the knees, and terminating in the formation of fetid purulent matter, which was found burrowing among the muscles of the leg.

Secondary abscesses were formed in the lungs; and matter was found within one of the ankle-joints, which was opened. In this case, Dr Barclay infers that the exciting cause of the *endocarditis* was manifestly that of general inflammation; but he adds, that it is very remarkable that this is the only instance under the age of twenty-five in which one set of valves alone is affected, and that these valves were the subjects of congenital malformation. The history of the case, he thinks, forbids its being regarded as allied to acute rheumatism, though the inflammatory attack began in the neighbourhood of the ankle-joints.

The other instance of endocarditis occurred in the person of a man aged thirty-nine, who, though a carrier, had never experienced any dyspnœa or irregular action of the heart, nor any incapacity for exertion, till four months previous to admission, when he had caught violent cold, with acute pain in the left side, for which blistering and other remedies were employed. Dropsical symptoms appeared three or four weeks before admission; and a rough murmur accompanied both sounds of the heart over the seat of the aortic valves. He had suffered no other previous illness that he could remember. On inspection, no lesion of any importance was found, except the rudimentary state of one of the semilunar valves, and extensive atheromatous deposit in the aorta, with destruction of its lining membrane. There was considerable congestion of the lungs, with pulmonary apoplexy; and a small tubercular mass occupied the apex of the left lung.

In 38 the two semilunar valves were much thickened from previous inflammation; and one was perforated by ulceration, so as to permit free regurgitation of the blood. The lungs were congested with some pulmonary apoplexy. The history of this case was similar to the preceding one; palpitation having commenced three or four months previous to admission, without the circumstance of any previous illness, and dropsy supervening, on account of which he was led to apply for admission to the hospital.

In the other case (20) the patient died in consequence of the effects of delirium tremens, and no history of the cardiac disorder is preserved.

In the following case endocarditis was complicated with jaundice.

A girl aged eighteen, pale and cachectic, stated to have been subject to palpitation of the heart from the period of childhood, had, four months previous to admission, an attack of acute rheumatism, with pain in the chest, since which event cardiac symptoms had greatly increased. Enlargement of the heart, with a mitral murmur, was manifest. Three or four days after admission, vomiting commenced, and continued without intermission till within one or two weeks before death. Two or three days after the beginning of vomiting, jaundice ensued; and the symptoms of affection of the heart became evidently and painfully aggravated. The condition of the patient precluded minute examination of that organ; but one week after admission pericardial friction was distinct, and obscured the other sounds of the heart.

Twelve days after admission, she is described as lying almost unconscious, with a brightly yellow skin, disturbed by frequent efforts to vomit. The precordial region had become more prominent and full since admission; the heart's action was very violent and irregular, shaking the whole upper part of the body, having evidently become much more labouring as the jaundice has supervened. Erysipelas of the face came on a few days before death, with an inflamed and ulcerated condition of the fauces and pharynx.

Inspection showed that the jaundice was dependent on disease of the liver, with obliteration of the secretory apparatus. One kidney had a patch of yellow discoloration covering about one-fifth of its surface, and penetrating to some depth into the cortical substance. The mitral valve showed traces of disease of long standing, and also of recent endocarditis; and on the aortic valves were slight fibrinous fringes. The pericardium contained several ounces of serum, with flakes of lymph suspended in it.

It is not known whether during life, in this case, the urine was coagulable or not. Dr Barclay believes that it was not. The point of interest he considers to be, that the predisposition to inflammation of the endocardium being given by previous disease of the mitral valve, a connection between the sickness and the development of endocarditis and pericarditis, and a mutual dependence of both these on the structural disease of the liver, and the presence of bile in the blood, seemed to subsist. Jaundice appears, on the other hand, to be in certain circumstances produced in the course of an illness, of which the most prominent character is disease of the heart, in consequence of secondary congestion of the liver, from an impediment to the circulation, particularly when this extends to the right side of the heart.

If a poisonous state of the blood is to be admitted as a cause of endocarditis, and of this acute rheumatism and granular degeneration are supposed to furnish examples, Dr Barclay conceives that it forms a just subject of inquiry, to consider why, in the one instance, the inflammatory action is so liable to extend to both sets of valves, while the other exhibits not any marked deviation from the ordinary statistical results which are here obtained as to all the forms of cardiac lesion together; whence proceeds the liability to a recurrence of endocarditis, when no subsequent affection of the joints has been observed; and why, in the last stages of granular degeneration, when epilepsy or coma marks the transmission of the poison to the brain, recent inflammation is so rarely found, and evidence of previous disease is of such constant occurrence. It is sufficient to say, that these are the inferences to be deduced from the statistics now presented.

It is manifest, that the only proof of inflammation affecting the valves, admissible in an inquiry like the present, is that derived from the presence of recent fibrinous effusion found in the dead body; but it is possible that, in an inferior degree, the thickening which is invariably found when inflammatory action is present, may of itself be the only effect produced. This conclusion seems to be almost necessary, if the rarity of instances of fibrinous deposit after the age of thirty-five years be considered. All the conditions recognised as causes of endocardial inflammation being found, simple thickening or opacity of the valve may be the only form of lesion; and it becomes a subject of inquiry whether this may not be the result of recent inflammation in a smaller degree. Again, the rarity with which fibrinous deposit has been traced to granular degeneration of the kidneys, among the large number of cases in which these organs and the heart are simultaneously affected, seems to point to the same possibility. With these facts the author is inclined to associate an observation which he has occasionally made, namely, the presence of a state of excessive vascularity, or congestion of the valvular apparatus, which was, nevertheless, not such as to justify its being regarded as a condition of disease.

After thus examining all the instances of recent endocarditis in the order in which they are arranged in the list, Dr Barclay gives the following arrangement of them according to the supposed cause of the development of the disease, with the most important complications.

- 6 are due to inflammatory disease—
  - 1 to recent rheumatic fever, } complicated with pericarditis, pleurisy, and pneumonia.
  - 1 to severe pneumonia, with previous rheumatic fever, } complicated with pericarditis, pleurisy, and pneumonia.
  - 1 to diffuse cellular inflammation of mediastinum, with previous rheumatic fever, and large and mottled kidney.
  - 1 to pleuro-pneumonia, with pericarditis, and large and mottled kidney.
  - 1 to inflammation of brain and lungs.
  - 1 to diffuse cellular inflammation, with purulent pericarditis, and deficiency of one semilunar valve.
- 5 are due to previous valvular lesion, without dropsy—
  - 1 to deficiency of one semilunar valve, with purulent pericarditis.
  - 2 to thickening, and atheromatous disease, one complicated with pleurisy.
  - 1 to previous rheumatic thickening, with pericarditis.
  - 1 to rheumatic thickening, and calcareous deposit, with pericarditis (apparently due, in some measure, to jaundice).

3 are due to valvular lesion, with dropsy—
2 to rheumatic thickening, both complicated with pleurisy, and one also with pericarditis.
1 to rheumatic thickening, and atheromatous disease.
In 2 there was valvular lesion, with albuminuria and dropsy—
1 due to rheumatic thickening, and granular kidney.
1 to granular kidney, and atheromatous disease.
In 1 there were valvular lesion and hæmaturia—
Due to calcareous deposit, and large and mottled kidney.
In 2 there was albuminuria, without dropsy—
Both due to atrophied kidney, one complicated with pericarditis.
In one there was albuminuria, with dropsy—
Due to atrophied kidney, complicated with previous thickening, and pleurisy.

The instances of disease of longer standing offer not any points of interest, when viewed in connection, except it be the mode in which one form of disease may be superadded to another in the same valve. To this circumstance reference was already made, in regard to those instances in which calcareous and fibrinous deposits were found together, when it was shown that atheroma might be the cause of endocarditis. Another condition, nevertheless, seems to be required,—one in which a valve having previously undergone an attack of acute inflammation, becomes afterwards the seat of atheromatous disease. The most certain evidence of this condition is derived from cases proceeding from a previous attack of rheumatic fever, the effect of which is known to be, in the first instance, to produce inflammatory thickening alone. In these circumstances the subsequent deposition of calcareous matter seems to be a mere coincidence, from the fact, that the proportion of cases of rheumatic origin, which present traces of atheroma, is only very slightly greater than that derived from all forms of diseased heart together; and this excess probably arises from the circumstance, attendant on inflammatory action in all parts of the body, that it frequently serves as a predisposing cause for the development of special maladies, of which tubercles and cancer may be adduced as examples.

The difference the author represents in the following manner:—

All cases of diseased heart give a proportion of	45 per cent.	of atheroma in general.
	25 ...	of atheroma of valves.
Instances of diseased heart connected with rheumatic fever give a proportion of	53 ...	of atheromatous disease in general.
	27 ...	of atheroma of the valves.

These facts, in regard to cases of rheumatic origin, prepare for the inference that the instances, in which a greatly indurated condition of valve is associated with a slight deposit of atheromatous or calcareous matter, were originally examples of inflammation of the endocardium, followed, after partial recovery, by the development of the specific malady. Cases of this order occur pretty frequently among the instances of atrophied kidney.

In seventeen cases disease of the liver is recorded as having been present, for the most part, in the contracted and granular form (Kirrhosis). This Dr Barclay was led to note, from the apparent connection in the case already detailed, between the supervention of jaundice and the excitement of renewed endocarditis; but he has not been able to draw from these cases any inference of value. In eight instances thickening of the tricuspid valve is mentioned, yet without throwing any light on the subject of affections of the right side of the heart. Their comparative rarity is well known, as well as their constant connection with obstructions of the orifices on the left side.

The deductions resulting from the statistical facts now communicated, may be stated in the following manner:—

1. Liability to double valvular disease is a consequence of rheumatic endocarditis; and this disease may attack at an early period of life, and terminate fatally at an early period.

2. A decided limit appears to be set by age to fibrinous deposit on the valves of the heart.

3. Atheromatous disease affects not by preference the mitral valve, as has been sometimes stated.

4. Though endocarditis is most commonly an acute disease, originating in connection with acute rheumatism, yet it appears that the endocardium or lining membrane of the heart is liable to become the seat either of chronic inflammation and its effects, namely, thickening and induration with corrugation of the valves, and arctation of their apertures, or of atheromatous deposition.

5. After this condition of the endocardium and valves has been insensibly established, an acute form of the disease may supervene, or hypertrophy or dilatation may ensue.

6. A large proportion, that is, nearly one-half of the fatal cases of granular kidney, is found coincident with valvular disease; but valvular disease is rare in those cases in which the kidney is large and mottled.

7. As a corollary from deduction 6, it must be admitted as probable, if not certain, that the large mottled kidney is a different disease from the granular or hard, and usually small kidney.

Points requiring farther investigation are the following:—

1. Whether the double valvular affection be the immediate, or the remote consequence of rheumatic endocarditis; and from what circumstance the disposition to renewed attacks of the disease is derived?

2. Whether endocarditis ever proceeds from the enlarged and mottled state of the kidney as its sole cause; and why, among so a great a number of cases connected with granular kidney, the inflammatory action was not more frequently found recent?

ART. V.—*On the Minute Anatomy of the Emphysematous Lung.* By GEORGE RAINEY, M.R.C.S., Demonstrator of Anatomy at St Thomas's Hospital. (From the Medico-Chirurg. Trans., Vol. xxxi. 1848.)

THE form of emphysema, which furnishes the subject of the following details, is that which is called Vesicular Emphysema; and the specimens of this disease, which have been selected for minute examination, are of the ordinary kind.

The subject from which the greater part of the preparations illustrating the facts described in this essay, and from which the drawings accompanying it were taken, was about forty years of age, and the general aspect of his lungs, especially in the vicinity of the emphysematous part, was healthy. There were, however, in some parts of the lung, a few small tubercular deposits.

Before the abnormal condition of the structures entering into the composition of the lungs, as they appear in emphysema, is described, a few observations on the normal state of these parts may advantageously be premised; and, the air-cells being the seat of this disease, these observations will be confined to them.

The only two structures entering into the composition of the air-cells, distinguishable by the microscope, are capillary vessels, and the membrane by which they are invested and connected together. The capillaries are known by the minute oval spots on their coat, generally considered to be the nuclei of cells, as well as by the extremely faint outline of the coat itself. In the uninjected subject this coat can with great difficulty be distinguished, especially when the capillaries have no blood in them; but in the injected lung it is sufficiently evident in those vessels which contain only a small quantity of injection. Nerves are not recognisable in the air-cells, although it can scarcely be imagined that no nerves exist in these parts.

If a portion of injected lung, magnified twenty or thirty diameters, be viewed by reflected light, it is seen to be made up almost entirely of irregularly-shapen cavities—the air-cells—differing very much both in size and form, but for the most part cuboidal. The dimensions of the air-cells differ greatly in different parts of the same lung, being the largest in those parts the most remote from its centre. They are frequently so large at the margins and extremities of the lobes, that these parts of a lung, retaining a good deal of air after death, are sometimes considered to be emphysematous when they are perfectly healthy. The walls of these cavities appear by reflected light to be formed entirely of a dense plexus of capillaries, consisting only of one layer of these vessels, which is so situated with respect to contiguous air-cells, that the same layer forms a part of the two cells between which it is situated, one side of it bounding one of these air-cells, and the other side the adjacent one. The air-cells communicate by large circular openings, through which the air can pass freely from one into another. An indefinite number of these air-cells, surrounded by areolar tissue, and supplied by a branch of the pulmonary vessels, consitutes a lobule. The larger branches of the pulmonary artery and vein run in the intervals between the lobules, while the smaller ramifications run between the air-cells themselves, and send off branches in different directions to the nearest plexuses, in which they anastomose very freely with the radicals of the pulmonary veins, and also with the ultimate ramifications of the neighbouring pulmonary arteries.

The membrane connecting the capillary plexuses—that is, the pulmonary membrane—is very thin, almost transparent, and made up chiefly of an irregular interlacement of extremely delicate fibres, which are most distinct around the openings of communication between the air-cells, where they appear to be somewhat circular. This membrane, whilst in a healthy state, is devoid of any regularly-formed corpuscles; the appearance of minute cellules may occasionally be observed in some parts of it; but these are so very rare, their form and size so irregular, and their situation so uncertain, that they cannot be regarded as an essential part of its structure, and may therefore be considered either as accidental or abnormal. The pulmonary membrane lines the air-cells, and in passing from one cell into another encloses the plexuses of capillary vessels between the two cells: hence between each two contiguous air-cells there is one layer of vessels and two layers of membrane. This membrane has no regular covering of epithelium, the ciliated form of epithelium ceasing with the bronchial membrane, which extends no further than the termination of a bronchial tube in a bronchial inter-cellular passage.*

The office of the pulmonary membrane is to connect and support the capillary plexuses, and to form the immediate boundary of the air-cells. It appears to be the seat of disease in emphysema, as hereafter will be shown.

Having premised these observations on the minute structure of the air-cells, it will be more easy to render intelligible the changes which they undergo in the disease now under consideration.

If a very thin section, or even a mere fragment of emphysematous lung, especially if it had first been minutely injected, be examined by a lens of one-quarter inch focus, by transmitted light, the pulmonary membrane will be seen to be perforated, or cribriform. The perforations in this membrane are generally well defined, of an oval or circular form, of various sizes, and more or less numerous, according to the progress of the disease. In very thin sections of lungs, in other respects normal, I have occasionally met with an opening or two through this membrane, corresponding to the areolæ of the plexuses, but these are so few, and their existence so uncertain, that they may probably be regarded as accidental, or perhaps in some cases the result of incipient disease, as I have not found them in lungs which I have known to be perfectly healthy, and which I have examined expressly for the purpose of determining the normal state of the pulmonary membrane. These openings cannot be confounded with those by which the air-cells open one into another, or by which the air-cells open into the intercellular passages, the latter being surrounded by a circle of anastomosing vessels.

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* See Medico-Chirurgical Transactions, vol. xxviii. p. 581, and Edin. Med. and Surg. Journal, vol. lxix. p. 204. January 1848.

The next point to be noticed is, that the pulmonary membrane in the vicinity of these perforations, as well as in many parts not yet cribiform, is studded with brightish spots, generally of a circular form, which vary in number and size, but in many instances are about the magnitude of the nuclei observable in the coats of the capillary vessels, and might possibly be mistaken for them, did they not occur in situations where there are no such vessels, as in the areolæ of the plexuses. These spots exist either singly or in clusters, and I have no doubt precede the formation of the perforations above observed, the latter having the form and occupying the same parts of the membrane as the former. The size also of the perforations accords with the size of the spots, being either that of an individual one or several aggregated together. This appearance, by a careful examination, is seen to be due to the presence of oil in the tissue of the membrane, or upon its surface. In some parts the oil appears to be so intimately blended with the substance of the membrane as only to increase its transparency, and thus to produce the appearance of bright circular spots; in others, where it is less intimately blended with the tissue of the membrane, it exists in the form of numerous minute globules, and of others of larger size, apparently due to the coalescence of these. This latter form presented by the oil occurs chiefly in those parts where the pulmonary membrane is thickest. If now a very small portion of the membrane thus affected be dried, then pressed between two pieces of glass, and gently heated, small particles of oil will be left upon the glass, distinctly visible by the microscope; or if it be digested in sulphuric ether, all the minute globules before discernible upon it will have disappeared.

From these facts it seems obvious that, owing to a change induced by some cause or other in the nutrition of the pulmonary membrane, the materials of which it is composed pass into a different state of combination, and are converted into oil, and that, in consequence of this change, the texture of this membrane in the part of the lung affected is in some places weakened, and in others wholly destroyed;—a condition which cannot fail so to impair the mechanical function of the membrane as to render it incapable of supporting the capillary plexuses, and of furnishing that resistance to the pressure of the air contained in the air-cells which is necessary for maintaining the several parts of the lung in their proper situation; and a condition, therefore, sufficient to account for those changes in the form and size of the air-cells, and their subsequent breaking up, which constitute the more obvious derangements in the structure in the lungs in emphysema next to be described.

The pathological appearances now to be noticed, are those observable in the disposition and condition of the blood-vessels. To determine the abnormal state of these parts satisfactorily, and to its fullest extent, the vessels of an emphysematous lung must have been injected prior to examination.

If those parts of an injected emphysematous lung, in which the air-cells are merely dilated, be examined by reflected light, the capillaries in their walls are seen to be smaller than natural, whilst the spaces circumscribed by their meshes are much increased in size. These changes in the vessels are proportional to the extent of the dilatation in the air-cells. They seem to indicate that the membrane, previously to its giving way, had been in a state of great extension. This extension may have arisen either from an undue pressure of the air in the air-cells upon the membrane, in consequence of some impediment to its free passage from the lungs, the membrane being in a perfectly healthy state; or from the pressure of the air in ordinary respiration, the membrane being weakened, and rendered more yielding than natural by some disease. In the case under consideration, I am not aware that there was any impediment to the respiration which can account for the changes which existed in the membrane and vessels of the lungs; whilst there seems to be sufficient cause for the elongated and attenuated state of these parts, in the process of fatty degeneration going on in the membrane, whose function it is to preserve all parts of the lung in their proper situation. I believe, therefore, that the dilatation of the air-cells and the changes in the capillary network were produced by the extension of their walls, which being weakened, as before described, have yielded under the pressure of the air within them, and caused the vessels to become elongated, their calibre diminished, and the areolæ of the plexuses to be enlarged. In parts where a visible lesion of the membrane between the air-cells has taken place, and several cells have become united so as to form one cavity, fragments of cell-walls are seen within it, containing extremely elongated capillaries and portions of plexuses, in which the vessels are very much contracted, and the areolæ enlarged, but mostly in length.

The extremely attenuated state of the vessels, prior to their division, seen in the injected emphysematous lung, is worthy of notice, as this state, together, perhaps, with a certain degree of retraction of their coats at the instant the division takes place, may be regarded as the reason that hemorrhage never occurs in emphysema of the lungs. After the disease has advanced still further, and the emphysematous cavity become large, and situated near to the pleura, all vestiges of the air-cells which were broken up to form it have in a great measure disappeared, and its sides are then held together by fibrous bands of various degrees of strength extending across it. These bands are the remains of the interlobular tissue, and they, like the pulmonary membrane, after sustaining a certain amount of elongation, give way. Their broken ends are distinguishable in these cavities in the form of irregular nodulated masses, produced by the retraction of the chords after their rupture.

The interlobular cellular tissue being gradually condensed during its elongation, becomes more compact and less capable of inflation; consequently the rupture of the bands into which it had been drawn out is not followed by extravasation of air between the lobules, and the occurrence of interlobular emphysema. When the emphysematous cavity has arrived close to the pleura, being separated from it only by the external wall of the most superficial cells, with the sub-pleural vessels and areolar tissue, the pleura in this part becomes thickened, and the sub-pleural vessels very much developed, though without exhibiting the appearance of injected vessels going to an inflamed part. These changes in the structure of the pleura and subserous cellular tissue are rendered necessary to support the pressure of the air, to which they are now exposed in consequence of the destruction of the air-cells lying immediately above them.

A review of the facts above stated seems to prove that the form of emphysema here described originates in a morbid process going on in the pulmonary membrane, by which the elementary substances entering into its composition are made to pass into that state of combination which is necessary to form oil,—a process very common in other structures and organs of the body, and which of late years has been described under the title of Fatty Degeneration.

Art. VI.—*Observations, Reflections, and Views on some Morbid Affections of the* Vena Portae; *and especially on Inflammation, Obliteration, and Ossification of this Vascular Trunk.* By M. Raikem, Titular Member of the Royal Academy of Medicine at Brussels. (Memoires de l'Academie Royale de Medecine de Belgique, Tome i. Bruxelles, 1848.)

I.—Instance of the Disease.

§ 1. *Clinical History; and Morbid Appearances.*

Jacques Loxhay, nailer, aged 57 years, born at Vaux under Chevremont, in the province of Liege, was admitted into the civil hospital of this city, (Brussels).

He stated that, in 1808, being a soldier at the siege of Flushing, he had been attacked by severe and obstinate ague, lasting during eighteen successive months; that, subsequently, the abdomen had always continued large; that he had jaundice, and, at intervals, swelled legs. He added, that for six months he laboured under incessant diarrhœa, accompanied with dull pains in the abdomen.

The condition in which he was when he was first subjected to observation was the following.

The abdomen was very much enlarged; a considerable amount of serous fluid was effused within the peritoneal cavity; the inferior extremities were greatly infiltrated. Nothing abnormal was recognised in the circulating and respiratory organs. The tongue was covered by a whitish fur; there was anorexia. No alteration in the liver was ascertained by exploration. Deep-seated, dull pains were referred to all parts of the abdomen. From ten to fifteen liquid and bloody evacuations took place from the bowels in the course of twenty-four hours. The urine was scanty, and discharged in small quantity.

The extract of Cainca, diluted in fennel water, was administered; and the patient was placed on an appropriate regimen.

The size of the abdomen appeared to be a little diminished under the influence of this treatment, which was continued during the space of from nine to ten days; but the alvine motions, which continued to be bloody, became more frequent and more copious.

At length, on the 20th of July, thirteen days after his entrance into the hospital, the patient had a little delirium, and died in the course of the day.

The body was inspected about forty hours after death, and presented the following state of the organs.

Infiltration of the lower extremities was considerable. More than thirty litres of limpid serous fluid was effused within the cavity of the peritoneum, the texture of which membrane, however, presented no appearance of alteration. The epiploons and the mesentery contained a large quantity of fat.

The intestinal canal was at the same time in a state of hypertrophy and hyperemia. It was of a uniform pretty deep red colour, manifestly proceeding from the imbibition of blood, which was particularly seated in the tunics subjacent to the serous membrane. Its walls, from three to four lines thick, were infiltrated by a great quantity of bloody serum. The mucous membrane presented in the greater part of its course a very intense red colour, and was sprinkled with ulcerations and several spots. He further observed on it a sort of reddish papillæ, prominent at its surface, which probably were only hyperemiated villosities. The infiltration of the intestines limited by the two parietal membranes, the mucous and serous, rendered the walls more thick, less pliant, and more resisting than usual, so that they could be easily divided by the scalpel. They contained a little black thick blood, and their internal surface was covered by a layer of the same matter.

The mesenteric veins were unusually dilated, and gorged with dark coloured blood.

The trunk of the *vena portae*, a little before entering the liver, presented a remarkable lesion, which spread into its principal hepatic divisions to the depth of from two to three inches. This vessel was more than one inch in diameter. Its external surface adhered by continuity, by means of an adventitious cellular tissue, to the contiguous surrounding parts, that is, to several branches of the gangliar nervous system, to lymphatic glands, to the hepatic artery, and to the cystic and hepatic ducts. At the spot corresponding to these adhesions, the vein was entirely obliterated by clots of blood intimately adherent to its inner surface. These clots were formed by a reddish white soft substance of little consistence, in which we could distinguish stratified layers of fibrine, irregularly disposed, and mingled with clots of blood more softened at the centre than at the circumference. In many points these layers adhered so firmly to the inner coat of the vein, that they could not be detached from it, without producing a breach of continuity; they filled not only the channel of the vessel, but they extended into its first divisions; then, in proportion as they were prolonged into these, they became gradually thinner, becoming more and more like coagulated blood. The thickness of the walls of the diseased vein was perceptibly diminished in several points, and increased in others. This thickening was caused by hypertrophy of the middle venous coat, which was of red colour, and presented an appearance of longitudinal fibres disposed in parallel lines.

The internal membrane, viewed in its whole course, appeared to present a decided thickening. Though it had been rendered neither opake, nor vascular, nor softened, at least in a manner perceptible to the senses, it nevertheless was more easily raised from the subjacent membrane than ordinarily is the case, and from its surface arose bodies like villosities. Having examined by the microscope a small slip of this tunic, I thought I could distinguish in it small vessels, tortuous and ramified, greatly more numerous in the neighbourhood of the spots, where fragments of fibrinous concretions continued to adhere, and in those where the venous walls were attenuated. Its surface was sprinkled with some purulent globules. With the naked eye, and especially by the aid of a lens, we saw on a recent portion, or one newly dried, reddish arborisations, which were seated in the tunics lying beneath the inner coat, and especially in the cellular tissue between this and the proper venous membrane.

Further, the inner surface of the portal vein was encrusted by more than twenty small scaly patches, hard, whitish, and with an osseous appearance. Of these patches the shape was irregular; the borders were angular, blunt, or rounded; the largest were one centimetre in length, and five millimetres broad; and the smallest were scarcely one millimetre in extent. Their thickness did not exceed one-third of a millimetre. By their circumference they appeared to be continuous, and confounded with the inner coat, all over which they were slightly elevated. Their adherent surface not only encroached upon this membrane, or rather the place which it occupied, but farther penetrated into the subjacent cellular layer, and appeared even to extend beyond that into the *tunica propria*. Their free surface was destitute of distinct epidermal covering.

The white hard calcareous matter of these incrustations was so thin that it appeared to be semitransparent. When they were placed between the eye and a strong light, it was possible to distinguish in them excentric curvilinear layers, disposed in parallel directions, but in an irregular manner, around a central nucleus.

Having subjected for more than twenty-four hours one of these osseous scales to the action of weak nitric acid, at first some bells of air were detached, then were formed whitish membraniform flocks; and, without undergoing any other change in figure, the patch was converted into a yellowish, soft, flexible substance, which, when examined by the microscope, presented a species of reticular framework, in which were seen fibres arranged very irregularly, and enclosing small irregular spaces, empty, variable in extent, and in which, undoubtedly, the calcareous matter dissolved by the nitric acid had been deposited.

The liver was manifestly atrophied, and smaller than usual. It weighed only thirty ounces, while in the normal state it weighs

in the adult forty-eight ounces. This organ presented no alteration in texture.

The gall bladder contained a large quantity of serous bile, yellowish and orange in colour. Its parietes were thickened, infiltrated, opaque, and whitish. The systic and hepatic ducts were filled with bile.

The trunk of the hepatic artery was three lines in diameter. This vessel contained no clots of blood; and the same was the case with the supra-hepatic veins.

The spleen was increased in size. Its surface was sprinkled with whitish fibro-cartilaginous patches, which affected at the same time its two investing membranes. The texture of this viscus appeared to me to be neither softened nor hyperemiated; it was, nevertheless, soft, resisting, and evidently hypertrophied. Its surface was united to the neighbouring organs by means of accidental cellular slips.

The skull was not opened. The viscera contained within the chest presented nothing unusual.

II. SOME OBSERVATIONS ON DIFFERENT POINTS RELATING TO THE PRECEDING CASE.

§ 1. *Seat and Nature of the Disease.*

On considering the different alterations found on inspecting the body of this patient, we find;—

Accidental cellular tissue establishing adhesions between the external surface of the portal vein and the surrounding parts, especially with several filaments of the hepatic plexus, with the hepatic artery, and the cystic and common hepatic ducts; atrophic attenuation of the walls of this vascular trunk in certain points, and their thickening in others, where were deep red coloration, and hypertrophy of the middle coat; thickening of the internal membrane, with villosities springing from its surface, and its coming away from the subjacent coat; fibrinous and bloody clots adhering to and filling the interior of the vessel; with all the other circumstances already enumerated. If these different anatomical characters be taken in connection, it is impossible to resist the conclusion, that an inflammatory process, probably followed by a subsequent attack, had affected the hepatic portal vein in all its tunics, and even the surrounding cellular tissue, and the surface of the contiguous parts.

It is known that contemporary anatomists are not agreed upon the exact nature of the common membrane of the vascular system. Some considering this membrane similar to the epidermis, deny in it any sort of organization, maintaining that it receives neither capillary vessels nor blood; and from this they infer that it is not susceptible of inflammation. (Bichat, Fohmann, Panizza, Rasori, &c.) Others consider this membrane as a species of serous tissue provided with epithelial layer, and several other organized layers superimposed, in which are discovered fibres, vessels, and even blood-vessels capable of being injected (Mascagni, Ribes, Berres, Henle, Tomassini, Burggraeve, Mandl, &c.) These authorities allow that it is susceptible of inflammation. Which view, it may be asked, is the most deserving of being adopted between these opposite opinions supported warmly on each side?

Admitting, as a point fully demonstrated, that the inner tunic of the blood-vessels is not solely and exclusively constituted by an epidermal layer, but that several fibro-vascular layers superimposed concur in its formation; considering also, that in the attempts made to detach this membrane, it may be very difficult and even impossible to isolate completely the epithelial covering from the subjacent layers, without carrying away with it portions of the cellular layers, which adhere closely to the internal surface of this membrane; the question may assuredly be asked, if the contradiction which prevails on this subject be not more apparent than real. At most, the complex structure of the common membrane of the vascular system appears to me to be sufficiently justified by the morbid changes to which it is liable. The principal of these are opacity; rugous portions; thickening; softening; suppuration; ulceration; fibrinous and pseudo-membranous exudations from its free and adherent surfaces; deposits of blood or of purulent matter formed between it and the subjacent tunic; the unwonted development of villous processes at its internal surface; the adventitious formation of a vascular blood-carrying plexus, which, after the previous softening and dissolution of the epithelial covering, penetrates occasionally to the false membranes by which it is covered; accidental cellular tissue, constituting organised slips and bridles, which establish adhesions between the opposite points of its internal surface, and give rise to contractions, agglutinations, obliterations, and similar changes.

§ 2. *Obliteration of the* VENA PORTÆ.

Obliteration of the Portal Vein, and of its primary divisions in the liver, by means of clots adherent to the inner surface of these vessels, presents another example of that species of lesion which M. Cruveilhier, Dr Robert Lee, Lobstein, and several other authors regard as a frequent consequence of phlebitis. To this opinion I have given expression on similar concretions, more than thirty years ago, in a Memoir on Inflammation and Obliteration of the Veins, which I addressed at Florence to the society established at the School of Medicine at Paris, of which I had the honour to be a corresponding member.

"It is undoubted," I observed in that performance, "that inflammation of the veins cannot take place without some lesion in the vital properties with which they are endowed, without disturbance in the action of their walls on the blood which they are entrusted with conveying and moving. May it not then be that there takes place in an inflamed vein, something analogous to what takes place in the case of inflammation of the intestines? Probably the blood in that part of the vein where the morbid affection is situated, undergoes a retrograde movement, caused by the perversion of sensibility and tonic oscillations in the vascular walls, the absence of blood, and the substitution of whitish purulent matter in its place. These facts have several times been ascertained in the entire course of an inflamed venous branch, though the calibre of the vessel may be neither effaced nor contracted at any point. Do not these facts therefore speak in favour of a similar supposition, by aid of which it may be possible to explain how far the blood contained in one *vena cava*, even in one inflamed portal vein, being incessantly kept in balance between two forces which communicate to it an opposite direction, must stagnate, be accumulated, stopped, decomposed in the interior of the vessel; and, lastly, be converted into a fibrinous substance, which, having become fixed and permanent, might have contracted intimate adhesions with the internal surface of the inflamed vein, lined as it is by albuminous exudation?"*

In a memoir relating to three cases with clinical and anatomico-pathological observations on phlebitis, addressed from Voltera in 1835, to the Royal Academy of Medicine at Paris, I made the following remarks. "In phlebitis it is certainly not uncommon to find purulent matter in one vein, or in one portion of a vein, the texture of which presents no trace of inflammation, as was seen in the upper part of the cephalic vein of the patient Lombardi; and similar instances place beyond doubt the conveyance of purulent matter produced or secreted by an inflamed vein, into another vein not inflamed which is above it, and accordingly the tendency of this fluid to be directed towards the heart, and to be poured into the current of the circulation; though, according to M. Ribes, the circulation ceases in the inflamed vein; the blood contained in it is more or less decomposed; and there is formed purulent matter which becomes retrocedent, because the blood is not moved there from the branches towards the trunk, but returns by a retrograde motion from the trunk towards the branches. It may be said that the circulation there ceases for ever."†

However it be, I am of opinion that we ought generally to consider the obliteration of a vein, which cannot be referred to a cause existing without the vessel, or even within its cavity, as a result of phlebitis, in whatever manner that may have taken place; for it would be difficult to prove that obliteration of veins, which nature wishes to maintain pervious, can take take place independently of all inflammation.

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* Observations sur l'Obliteration et Inflammation des Veines. Addressées, le 2 Decembre 1812, a la Societe de l' Ecole de Medecine de Paris.

† Revue Medicale, 1824.

Of late years there have been recorded, relating to obliteration of the VENA PORTAE, several facts, which have formed the matter of consideration to French, German, English, and Italian physicians, mostly contemporary.*

When a large vein is obliterated, the circulation may yet in some instances be carried on by means of the anastomosing branches, which then acquire a development more or less considerable. It, nevertheless, happens not always in such a case, that the collateral anastomosing veins are dilated to the extent of being able to continue the circulation, which is impracticable in the main trunk, in the same manner as dropsy does not constantly ensue in those parts from which proceed the afferent venous rootlets; because the lymphatic vessels and the collateral anastomosing branches may in certain circumstances be adequate to maintain the course of the circulation.

The same is true as to obliteration of the portal vein. There is established in that case a supplemental collateral circulation, most commonly insufficient or incompatible with perfect health, analogous to that which takes place in the collateral anastomosing arteries, when the trunk of these arteries ceases to give passage to the blood.

I may recal here what I stated on this subject in 1812:—"As to the manner in which the return of the venous blood of the extremities may be effected during life, in individuals in whom the lower portion of the abdominal *cava* is obliterated, it is probable that the dilatation of the numerous communicating branches established between the veins of the lower extremities and the lumbar veins, the epigastric veins, the branches of the *vena demi azygos*, and the portal vein, supply in some manner the function of the obliterated venous trunk. On the other hand, it is probably to the conveyance of a larger quantity of blood by the portal vein than is normal, that is to be ascribed the excess in nutrition which the liver presented in the subject of my first case, and in the instance of the Hollander, whose remarkable history Bontius has recorded in the third book of his Natural and Medical History."†

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* In France,—Bouillaud, Dance, Reynaud, Duplay, Andral, Cruveilhier, Breschet Gendrin, Ribes, Raciborski, Fauconneau-Dufresne, Gintrac, Lambron, &c. In England,—Baillie, Hodgson, Robert Lee, Stokes, &c. In Germany,—Puchelt Bailling, Mohr, Schoenlein, Baczynski, Kaether, Sander, Messow, &c. In Italy, —Testa, Meli, Brera, Tomassini, Crescimbeni, Benvenuto, &c.

† Observations sur l'Obliteration et Inflammation des Veines. Addressées l' 2d Decembre 1812, a Societe de l'Ecole de Medecine de Paris. Par M. Raikem.

The labours of several modern anatomists, as Wilson, Cruveilhier, Breschet, Retzius, Schlemm,* allow us not to doubt, that in the normal state there exists a great number of anastomoses between the general venous system and the system of the portal vein. By means of these communications, the circulation of the blood may still be carried on in those organs in which the rootlets of the portal vein arise, when this vascular trunk can no longer be traversed by blood; and then it sufficiently often happens, that the veins of the abdomen and of the chest, both external and internal, and perhaps also those of the spine which communicate with the intestinal veins, become hypertrophied, and are dilated and developed in a remarkable manner. These changes are illustrated by the facts of the following case.

There was admitted into the hospital of La Charité at Paris, a man presenting on the whole anterior part of the trunk numerous large venous ramifications, and considerable abdominal dropsy. Notwithstanding the continuance and even the increase of appetite, emaciation made rapid advances. The quantity of fluid accumulated within the abdomen rendered necessary one operation of parakentesis, which shortly after was followed by two other operations. The dilatation of the superficial veins, and the speedy reproduction of the fluid, led to the inference that there was an obstacle to the intestinal venous circulation. The development of the whole venous system of the walls of the abdomen, and of the anterior part of the chest, was enormous. In each iliac region arose a trunk the size of the little finger, divided into numerous large branches, anastomosing many times with each other, and with those of the opposite side, and forming a plexus with large meshes, the numerous branches, of which partly proceeded to the axillary regions. The lower extremities were infiltrated, while the upper extremities were dry and emaciated. The belly, though very much distended, was in no degree painful. The skin was dry and void of heat; the pulse regular and without frequency; the tongue moist and red; the thirst moderate; the appetite not remarkable; the abdomen relaxed; the urine red and scanty. Pulmonary catarrh took place, and very soon extreme feebleness and depression followed. Lastly, the patient fell into a comatose state, and expired at the end of twenty-four hours, six days after his entrance into the hospital.

It was expected that we should find an obliteration more or less complete of the portal vein or the *vena cava;* and to this point the necroscopic researches were directed.

The right branch of the portal vein, and all its divisions in the right lobe of the liver, which was found in a state of remarkable atrophy, were entirely obstructed by a solid substance very firmly adherent to their walls, and presenting to the lens the aspect of the yellow tissue of the arteries.

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* Histoire des Descouvertes relatives au Systeme Veineux; Memoires de l'Academie Royale de Medecine de Paris, Tome ix.

This change was prolonged into the corresponding hepatic veins. The inferior *vena cava* itself was very solidly obstructed to the extent of three inches, measured from its opening into the right auricle. The material cause of this obstruction was the presence of a substance similar to that found within the portal vein, and, particularly, considerable and very irregular thickening of the veins. Outside the *vena cava*, above the opening of the hepatic veins, was an irregular mass formed of matter analogous to that which obstructed the vessels. The left branch of the *vena portæ* was filled with fluid blood, and its ramifications within the liver had a larger calibre than usual; the same was the condition of the corresponding hepatic veins, of which one trunk opened into the *vena cava* near the auricle. The enormous veins ramified under the integuments of the trunk, arose from the epigastric veins, and proceeded to join some of the trunks of the intercostal veins; others, the axillary veins. The volume of these veins was considerably increased in the same manner as that of the azygos vein. Thus, a great part of the blood which goes to the heart by the inferior *vena cava*, entered that organ by the superior *vena cava* through the channel of the collateral intermediate trunks. In the right auricle of the heart opened directly a trunk without analogue in the normal state, and which was composed of a multitude of branches issuing from the liver or the neighbouring parts. Lastly, the large coronary vein had the size almost of the crural vein, and received a great number of infra-diaphragmatic and other branches. The veins of the intestines in general were dilated. The alimentary canal contained a great quantity of matter resembling coffee grounds.

The volume of the spleen was enlarged.

A considerable quantity of serous fluid was found within the ventricles of the brain.

This observation, which is in many respects interesting, belongs to M. Reynaud, and was published in 1829 in the *Journal Hebdomadaire.* It furnishes an example, as has been seen, of obliteration of the right branch of the portal vein, and of the inferior *cava*, which, as a twofold organic cause, had produced the extraordinary development of the collateral anastomosing branches of these two venous trunks.

But the question may be proposed, does a similar venous development upon the abdominal parietes likewise take place, and in a manner as strongly marked, in all instances in which the trunk of the *vena portæ* is alone obliterated?

On this point M. Fauconneau-Dufresne, author of an important memoir on Inflammation of the Abdominal Venous System, inserted in 1838 in the *Encyclographie* of the Medical Sciences, observes. "I know only five instances of complete obliteration of the trunk of the portal vein. Three cases belong to M. Bouillaud; one to MM. Reynaud and Duplay; and the fifth to M. Duplay alone. In the two last only mention is made of this venous development of the abdominal parietes. In the instance recorded by M.M. Reynaud and Duplay, on the belly were enormous varices, the rupture of which was a subject of apprehension; in the instance given by M. Duplay, large veins like crow quills formed numerous anastomoses at the lower part of the abdomen; two of these veins, a little larger than the others, ascended on each side of the *recti* muscles towards the thoracic parietes."

There is, nevertheless, no doubt of this external venous development in the three instances given by M. Bouillaud. M. Gendrin did not observe it in the instance of obstruction of the *vena portæ*, described in the first volume of his treatise on Practical Medicine. Dr Mohr makes no mention of it in the instance which he relates of severe inflammation of the portal vein, which terminated in suppuration in a young domestic servant, aged seventeen years. M. Ernest Lambron likewise failed to observe it in the person of a day-labourer, attacked with inflammation of the same vein, produced by a fish-bone which had traversed the pyloric extremity of the stomach. Inspection showed, nevertheless, that the trunk of this vein was obstructed by false membranes; that the portal sinus was filled with purulent matter; that some of its hepatic divisions were filled with a puriform wine-lee-like liquid, with their walls in some points sound, in some inflamed and thickened, and presenting in some spots false membranes of a slate-grey colour; others contained only clots of blood which were prolonged into the most minute venous ramifications.* Neither is any mention made of this venous development in the extract which the *Encyclographie* of Medical Sciences has published, of a history read by M. Gintrac to the Royal Academy of Medicine at Paris at the meeting on the 11th April 1843, and which is that of an old soldier who had died in consequence of obliteration of the portal vein. In this instance the walls of this vessel were ossified immediately before their entrance into the liver. Nothing similar was seen by M. Andral in a case in which there was a large mass of lymphatic glands degenerated into tubercles, which on the one side compressed forcibly the *vena cava*, and on the other the portal vein.† Lastly, I did not observe any external venous development in the body of the nail-smith who was examined in my presence.

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* Archives Generales des Medecine, Juin 1832. Craigie's Pathological Anatomy, 1848, p. 879 and 880.

† Clinique Medicale. Tome iv.

From so great a number of facts, I conceive that I am entitled to conclude that, in the instance of obliteration of the portal vein, the circumstance of external venous development is far from being constant; that it is not manifested even in the majority of instances; and, therefore, that we ought not to assign to it great diagnostic value, since it may exist in a very marked degree in the absence of all obliteration of the abdominal venous trunks, as is proved by the case of the old soldier mentioned by M. Cruveilhier, and in whom there was a dilatation of the abdominal subcutaneous veins, accompanied by a large communication of these veins with the *vena portæ,* through the channel of the umbilical vein, which had preserved the calibre, which it presents in the fœtus.*

If, however, the abnormal development of superficial veins in the abdominal and thoracic parieties do not often accompany the obliteration of the trunk of the portal vein, the same cannot be asserted of the development of the internal veins of the abdomen.

In the majority of instances, on the other hand, these vessels undergo an extraordinary degree of dilatation. Of this fact complete evidence may be obtained by considering the observations cited and reported in monographical essays on inflammation of the portal vein, recently published at Zurich and at Berlin, by Doctors Baczynsky, Sander, Kaether, Messow, and in those for which we are indebted to MM. Bourdon and Piedagnel, Gintrac, Lambron, Andral, Gendrin, and other observers.

§ 3. *Influence exerted by Obliteration of the Portal Vein in the Secretion of the Bile.*

On examining the body of the man Loxhay, I ascertained that the gall bladder and the hepatic and cystic ducts contained a sufficiently large quantity of serous bile of an orange yellow colour; and on this occasion I made it the subject of inquiry, if the presence of bile in abundance, in this receptacle and in its existing ducts, indicates that the liver fulfilled during life the secreting function which is assigned to it.

In several similar instances in which the hepatic trunk of the portal vein was obliterated to such a degree, as no longer to permit the blood to traverse it in order to go to the liver, it was nevertheless possible to be convinced, by necroscopic inspection, that this gland had, nevertheless, secreted bile, the elements of which might have been furnished by the hepatic artery, which had been left pervious and entire.†

In the *Journal Hebdomadaire*‡ is found the instance of a woman, aged fifty-six years, who died in consequence of chronic inflammation of the portal vein and medullary fungous formations dispersed in the lungs. The trunk and branches of this vein were partly obstructed by clots, partly filled with cream-like reddish puriform matter. The hepatic duct, nevertheless, contained bile, but there was none in the gall bladder, because the cystic duct had become impervious in consequence of agglutination of its walls, and because the neck of this membranous sac was obliterated by vegetations. At the inspection of an old soldier, the account of which M. Gintrac communicated to the Royal Academy of Medicine at Paris, and who laboured under complete obliteration of the portal vein of old standing, along with hypertrophy of the heart, the liver was fouud small, whitish, and as if mammillated at the surface, yet the gall bladder contained a yellowish liquid, not very thick.* Dr Mohr, who has recorded, as already mentioned, a case of suppurative inflammation of the portal vein in a young female, observed on opening the body, that all the veins belonging to the system of the portal vein contained purulent matter, albuminous exudation, and clots; but in the stomach and small intestine there was an albumino-bilious liquid and gases (ventriculus et intestinum tenue gasis et liquore albuminoso-bilioso referta sunt); proving evidently, that during life, the secretion of bile by the liver, and its conveyance into the duodenum by the biliary duct had been accomplished.†

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* Anatomie Pathologique, 16ieme Livraison, pl. vi.

† Brachet et Fouilhoux, Physiologie Elementaire de l'homme, Faisant partie de l'Encyclopedie des Sciences Medicales, p. 85. Paris, 1833.

‡ Journal Hebdomadaire, Fevrier 1830.

In the instance of inflammation of the portal vein which M. Schoenlein observed in 1829 at the hospital of Wurtzbourg, inspection showed that this vein contained purulent matter, and was surrounded and covered, as well as the hepatic artery and the biliary ducts, by dense, thick, firm, and hyperemiated cellular tissue, analogous to the fibrous tissue of the *corpus cavernosum.* This accidental production was the means of contracting these excretory canals and the trunk of the portal vein to such a degree as to render them impervious. Hence, doubtless, proceeded the bloody engorgement and extraordinary varicose hypertrophy which were presented not only by all the veins which contribute to form the *vena portæ*, the gastric coronary veins excepted, but also the external abdominal veins, as well as those which come from the epigastric veins, and proceed to the axillary veins, and the internal mammary vein. Notwithstanding, however, the obstacles which were opposed to the current of blood in the portal vein, the liver presented the most perfect state of integrity in all its parts.

In order to afford fuller confirmation to the inference which has been deduced from the case recorded by me, and to prove that the liver may continue to secrete bile, even when it ceases to receive blood from the portal vein, I shall mention a fact quoted by M. Dance, and which relates to an attack of inflammation in the ventral portal vein, which had succeeded disorganization of the biliary duct and some of the neighbouring veins. In this instance the interior of the liver, and especially its surface, "were sprinkled with purulent deposits formed by a mass of veins filled with purulent matter, which veins were continuous with the rootlets of the *vena portæ hepaticæ.* Several branches, and even the trunk of this vein, were filled with puriform pultaceous matter of a yellow colour, analogous to that of bile, and mixed with liquid blood, black, or decolorized clots, free or attached; and the gall-bladder was filled with serous turbid bile,—presenting towards its fundus four small blackish rounded ulcerations, which affected the whole substance of the inner membrane of this receptacle. The biliary duct was destroyed throughout its whole extent, and converted into one oblong anfractuous cavity, containing membranous shreds detached from its walls and penetrated with bile.*

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* Seance du 11 Avril, 1843.

† F. Mesow de Inflammatione Venæ Portarum seu Pylephlebitide, p. 18 et seq. Berolini, 1841. Πυλη, *Porta* ; Φλεβς, *vena*.

It is known that some physiologists, relying on the testimony of Glisson, Fantoni, Fourcroy, Meckel, Schultz, and other authors, are of opinion that the blood of the portal vein is endowed with peculiar qualities, and contains particular materials, which render it exclusively proper for the secretion of bile. The statements made by these authorities, it has been observed by Duges, have been contradicted by a number not less considerable of observers equally competent; for instance, Walaeus, Harvey, Steno, Diemerbroeck, and Bichat. Though the facts which I have already specified, tend also to controvert this doctrine of peculiar qualities and ingredients, yet, as it may be the case, that I have not succeeded in shaking the firmness with which it is maintained, I shall here enter into some illustrations, which may show more clearly the truth of the proposition which has been already supported on the evidence of pathological anatomy.

It sometimes happens in the human body, that the system of the portal vein, in place of proceeding to the liver, and being distributed through that organ, is continued immediately with the general venous system, and joins the inferior *vena cava*, of which it then constitutes a ramification. Several authors have directed the attention of physiologists to anomalies of this kind; and several authentic examples of this deviation are known to be cited by M. J. F. Meckel, Duges, Isidore-Geoffroy Saint Hilaire, &c.

In the instance recorded by Abernethy, the portal vein opened directly into the inferior *cava* near the origin of the emulgent veins, so that there was no other blood-vessel proper to the liver except the hepatic artery, which consequently must have furnished the materials necessary to the secretion and the nutrition of the organ. Notwithstanding this, bile was poured into the intestines as usual, and it filled the gall bladder. The subject was a child about one year old.* It is to be observed that, in the instances relating to similar anomalies, the hepatic artery presented a calibre more considerable than in the normal state. This development of the hepatic artery might also take place when the *vena portae* has been obliterated for a long time; for in the subject of my case, this artery was three lines in diameter.†

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* De la phlebite uterine et de la phlebite en general.—Archives Generales, 7ieme Annee. Tome xix. p. 40 et suiv.

Further, Cuvier teaches, in his Lectures on Comparative Anatomy, that "all the MOLLUSCA have a liver in general of very considerable size, and that in these animals this gland receives not in particular, as it does in the vertebrated animals, the blood which has circulated through the intestines, and which has already acquired a venous nature. The liver in these animals derives from the aorta the blood necessary both to its own nutriment and to the formation of the liquor which it secretes; and the blood, after having circulated through the liver, returns to the *vena cava*, which in these animals unites with the pulmonary vein." Now, may it not be objected, that experimental inquiry in living animals gives a formal contradiction to a corollary, which I believe to be founded at the same time on pathological anatomy, on anomalies of vascular insertions in the human body, and upon pathological anatomy?

Boerhaave, Soemmering, and Simon, indeed state that, when a ligature is applied to the portal vein in dogs and other living animals, the secretion of bile is at the end of some hours completely suppressed. "Vivo Cani vinculo constringitur vena portarum, ita post paucas omnino horas supprimitur secretis bilis," says Boerhaave.‡ To these words, however, of his preceptor, Haller immediately adds the following note: "Non invenio; sed arteria hepatica ligata abunde tamen bilem excerni. Malpighi de Liene. Atque videtur, ligata vena portarum non bilis secretionem solam, sed vitam perituram."

On the other side, I remember that one of my former instructors, the illustrious Dupuytren, reposed scarcely any credit in the alleged success of these experiments, considering, he said in his Lectures on Physiology, that he had several times observed, that the ligature of the portal vein was followed by the death of the animals on which the operation was performed.

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* Philosophical Transactions. Year 1793.

† In his Manual of General and Descriptive Anatomy, J. F. Meckel quotes two authentic examples of this kind of anomaly,—one by Abernethy, the other by Lawrence; and MM. Meniere and Manec have seen instances in which the portal vein received branches from the crural vein.

‡ The blood which comes to the portal vein and the liver, proceeds in BIRDS and FISHES, not only from the Digestive organs, but partly from the abdominal parietes, from the posterior or sacral members, from the tail, and sometimes also in fishes, from the genital organs.—Burdach, *Physiologie.*

Further, Philip states,[*] that he had observed that the ligature of the *vena portae* performed upon living dogs,[†] is followed only by diminution, and never by complete suppression, of the biliary secretion. In their Treatise on Physiology, MM. Brachet and Fouilhoux assert, that they had attempted to enclose in a ligature sometimes the portal vein, sometimes the hepatic artery, in living animals; that it appeared to them that the ligature of the hepatic artery suspended the secretion of bile, and that the ligature of the portal vein did not prevent it; but that they did not venture to draw any positive conclusion, because the animals lived too short a time to permit any satisfactory result to be established.

Finally, in his treatise *De Liene*, published towards the middle of the seventeenth century, Malpighi expresses himself in the following terms:—"I think that it is probably to be inferred, that the constituent part of the bile or fluid conveyed through the biliary or the common duct into the intestines, is manifold, as it probably comes from different vessels and organs; for the larger portion of it which is most liquid comes from the minute glands of the liver, being conveyed into these by the minute branches of the portal vein, and not improbably from the extreme terminations of these arteries, which departing from the hepatic duct, terminate at length in the minute glands of the liver."[‡]

After so many researches undertaken in order to elucidate the question whether the secretion of bile belongs exclusively to the portal vein, or to the hepatic artery, as Gunzius and Swammerdam thought, or to both of these vessels, we are disposed at present to return to the opinion of the ancient Italian anatomist whose words I have above quoted, because, though we admit that the blood of the system of the portal vein is that which furnishes the materials for the secretion of bile, yet one of the most learned and most profound modern physiologists recognises that two orders of vessels are united in the capillaries of the liver. Thus the arterial blood[*] contributes to the secretion of bile in a secondary manner, either, as Autenreith says, by its oxygen attracting more forcibly the basic elements by antagonism, or by its producing the bitterness of the bile by oxydating carbon, or, in short, by its furnishing mucus to this liquid.[†]

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[*] Muller Archiv d'Anatomie. Burdach Physiologie.

[†] I think M. Duges is in error when he quotes Malpighi among the experimentalists, who state in their writings that they had applied a ligature to the portal vein in living animals.

[‡] Probabile recolligendum puto, bilis et seu humoris per choledochum vel communem ductum in intestina derivati componentem scilicet materiam multiplicem esse, quæ forte variis etiam excidit e vasis et organis; nam major copia quæ fluidior est, erumpit a glandulis hepatis, in ipsis delata a venae portae ramulis, et non improbabiliter, etiam ab extremis arteriarum finibus, quae a ductu hepatico, excidentes, tandem in hepatis glandulas terminantur. Exercitationes Anatomicæ de Structura Viscerum, cap. vi., p. 141. Amstelodami, 1668.

In the present state of physiological knowledge, therefore, there are reasons sufficiently conclusive to make it be thought, that the hepatic artery is capable alone of furnishing to the liver the materials neccessary to the secretion of bile, when the portal vein is obliterated to so great a degree, as not to allow the blood to be conveyed through it to this gland.

With regard to the nutrition of the liver, it is reasonable to propose the question,—is it indeed proved, as is generally believed, that the hepatic artery is alone charged with this function, to the exclusion of the portal vein, which would never contribute in any manner to this object? Might we not demand of those who, on this point, state no doubt why the liver is not atrophied and does not fall into gangrene in living animals, in which a ligature has been placed on the hepatic artery, and in those individuals in whom complete obliteration of this artery must have taken place a long time previous to the termination of life, as was witnessed, according to M. Gendrin, in a young woman of twenty-three years? Why do we observe that the liver is atrophied in those in whom the portal vein has been entirely obliterated for a long time?[‡] Lastly, why does it happen, as I have had occasion to observe, and as the example quoted by Bontius shows, that the liver is hypertrophied, when the inferior *vena cava* being obliterated, a large quantity of blood is transmitted by the portal vein, in consequence of the extraordinary development which the anastomosing branches then acquire?

Such are the doubts which have been suggested to me by the comparision of several anatomico-pathological observations. These doubts, it is possible that ulterior researches may entirely remove.

§ 4. *Ossification of the* VENA PORTÆ.

Among the pathological alterations which I found in the body of the subject of this memoir, the multiplicity of osseous hard incrustations, flattened, thin, whitish, semitransparent, irregular in shape and variable in dimensions, which lined the inner sursace of the trunk of the *vena portæ hepatica*, was certainly not the least deserving attention. This was, further, the first instance in which it happened to me to observe such a circumstance.

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[*] This opinion is that of Ferrein, of Marherr, of Adelon, and several other authors.

[†] Burdach Abhandlung de Physiologie.

[‡] See the history of the case of Loxhay at the beginning of this memoir, and the cases which I have quoted from MM. Reynaud and Gintrac.

I speak not here of *calculi* or of stony concretions situate in the interior of the vein, but of small patches of calcareous appearance, deposited, applied, incorporated, so to speak, in the walls of the vessel, and which Dr Phæbus refers to the form of petrifaction of the veins, which he denominates, *Concrementa in parietibus venarum nidulantia.*[*]

It was the belief of Bichat, that the common membrane of the vessels of the system of modena blood does not undergo ossification in the aged, as does that of the arteries. According to this anatomist, "its organization appears to resist the process of penetration by phosphate of lime;" and when this is observed, "it is an unnatural state, whereas ossification of the common membrane of the red blood is a natural condition in the aged."[†]

This opinion has undoubtedly been suggested to the great creator of general anatomy, by the generally acknowledged rarity of ossification of the veins.

It is, nevertheless, not necessary to exaggerate this rarity, which is rather relative than absolute. Thus, after having traced a sketch of our actual knowledge on this subject, Dr Phæbus mentions five instances of ossification of the portal vein; nine instances of ossification belonging to the system of the inferior *vena cava*; three instances of ossification of the veins forming part of the system of the superior *cava*; and two instances belonging to the veins of both systems. With regard to the pulmonary veins, Bichat saw several instances of their ossification. Observers have even sometimes met with osseous excrescences developed in the tunics of the pulmonary artery, and a callous and cartilaginous condition, even complete ossification of this vascular trunk.[‡]

MM. Bourdon and Piedagnel found an instance of complete ossification of the portal vein in the dead body of an individual in whom the spleen was very much enlarged; the stomach and intestines were everywhere injected and inundated with blood.[§] M. Gintrac read recently to the Royal Academy of Medicine at Paris the history of a case, in which there was also an instance of ossification of the same vein. In the Museum of Pathological Anatomy at Vienna, is a preparation of complete ossification of some veins situate on the lateral regions of the uterus; and in that of the Hospital at Venice, is an ossified femoral vein which was near a cancerous sore.[‖] In the anatomical collection of the Royal College of Surgeons at Dublin, is preserved a femoral vein, which presents at its inner surface a small osseous scale, and another instance of ossification of the veins which has its seat in the iliac and in the femoral vein.*

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[*] De concrementis venarum osses et calculosis. Berolini, 1832.

[†] Anatomie Generale, tom. i., p. 404. Paris an x.

[‡] Lobstein Traité d'Anatomie Pathologique, tom. ii., p. 555.

[§] Brachet et Fouilhoux Physiologie, p. 86. Tom. v. de l'Encyclopedie des Sciences Medicales. Paris, 1835.

[‖] Dubini Notizia succinta delle piu importanti preparazioni anatomico-pathologiche, &c. Annali di Medecina, Vol. cv. 1843.

It is known that Bichat and Meckel placed the seat of morbid or accidental ossification of the walls of arteries and veins in the internal membrane of these vessels. I doubt whether this opinion has at present many partizans among those who cultivate the study of pathological anatomy. I think even that the great part of modern authors are of opinion that this ossification begins usually by being developed in the cellular layer, situate between the internal and the proper membrane. Rayer, and a few others, only place the seat of these productions in the fibrous tunics of the arteries. It is also in this layer, according to M. Andral, that are formed the sub-globular concretions which are occasionally met with inside of veins. Instead of forming part of the walls of these same vessels, they thrust before them the internal membrane, and descend with it into the cavity of the vein; the membrane then contracts itself behind the concretion, and forms to the concretion a species of stalk or pedicle.

Here it may be proper to observe, that in the controversies which have arisen in our time on the seat of veinstones, and the mode in which they are developed, the ideas announced on the subject may be referred to one or other of the three following opinions. *First*, Some believe with Hodgson that these concretions are formed originally in the parts contiguous to the veins, and then penetrate, by means of progressive absorption, into the interior of these vessels. *Secondly*, Others, among whom may be mentioned Meckel, Rudolphi, and Phaebus, allege that veinstones are formed within small sacs or cysts, which are developed at the external surface of the internal membrane of the veins. The manner in which Phaebus expresses himself on this point in the essay already quoted, is shown in the following passage:—"Statuemus, concrementa subglobosa in externa (*adnata*) tunicæ venarum internae superficie nasci, crescentia hanc introrsum urgere atque protrudere, ita ut tandem parti protrusa pediculo modo tenui adhæreat.; denique absorpta parte protrusa vel rupto pediculo libera in vasis lumine jacere. Quam sententiam M. Rudolphi fovet, uti non tantum olim in ejus lectionibus, sed et nuper modo ea ipso audivi; et casus adeo evidenter ipsam probantes observasse sibi videtur." *Thirdly* and *lastly*, MM. Tiedemann, Otto, Carsewell, Heusinger, and Lobstein, think that veinstones are formed by the blood, or by its materials.

With regard to flat osseous incrustations of the veins, it is generally believed that they are seated between the internal venous membrane and that which is subjacent, and that they are formed as those of the arteries.

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* Houston, Pathological Catalogue. Dublin 1840.

Andral, who adduces in support of the hypothesis above mentioned examples of the change collected by Dupuytren, Beclard, and Breschet, finds the greatest analogy between the mode in which these pediculated concretions are formed, and that in which are produced those which are sometimes found within the articulations. He accordingly proposes the question, whether this be indeed the origin of certain calcareous concretions found within the veins in the midst of clots which fill up the calibre of these vessels; and after having entered into some considerations to this purpose, he concludes by admitting that it is possible that these concretions may have been formed in the blood itself, associating himself thus with the opinion of Tiedemann, who thinks that they cannot be developed and exist between the internal membrane and the middle membrane of the veins. Otto, Lobstein, Cruveilhier, and Briquet, are in like manner of opinion that these concretions are formed in the interior of the veins.

M. Hasse, the author of the Treatise on Pathological Anatomy, states that veinstones are formed by the successive deposition of calcareous matters in the layers of concretions of blood, and that this deposit proceeds originally from the blood. If, he says, we dry the blood-concretions formed in the venous dilatations, before they have yet contracted adhesions with the internal wall of the vessels, they shrink and become crisp; when they are divided by a cutting instrument, they grate under its edge; and we find in them already, contrary to all expectation, calcareous indurations. We see farther, veinstones in appearance quite hard, which, by desiccation, are reduced to very small bodies, irregular in shape, and presenting layers irregularly and imperfectly converted into calcareous matter. When, adds the same writer, we are satisfied with examining veinstones which have attained their last degree of development, it is impossible to give an account of their mode of formation, because in that case the venous tunics have already undergone in their neighbourhood, and even beyond that, a considerable change. To this cause, undoubtedly, we must ascribe the difficulty and embarrassment experienced by those who endeavour to prove that veinstones are formed between the two membranes of the veins.

One of the corresponding members of our academy,—namely, Dr Carsewell,—has also devoted himself to special researches, on the nature and formation of veinstones, the different stages of evolution of which he has attempted to elucidate in the plates illustrating his work on Pathological Anatomy. According to the observations of our learned colleague, a small clot of blood is first formed within the vein; then in the centre of the clot appears by successive degrees a small nucleus composed of concentric layers. Gradually the red matter of the blood is partly absorbed, and the fibrine assumes the physical characters which are peculiar to it. From this time it is observed that the fibrine possesses a tendency to be disposed in lamellated layers, the central one of which, and which is first formed, becomes manifestly calcareous. This induration is effected successively in different layers, until the whole is converted into a small solid mass. Sometimes there is found at the point situate at the most remote extremity of the vein, a small nucleus of blood, or of fibrine, which is converted into a concretion; and in this instance the veinstone is found to be provided with a small caudal appendix.

It becomes a question whether the ingenious theory of the formation of veinstones now explained in a summary manner, is applicable to the species of calcareous concretions, which are found at the internal surface of the walls of the portal vein in the individual who forms the subject of my case, on the understanding that these concretions proceeded originally from the blood, or from its materials.

I feel no inclination to undertake the solution of a problem so difficult. But when I consider on the one side,—1*st*, the disposition of the stratified layers which constitute these cretaceous plates; 2*d*, the absence of every indication of epithelium at their free surface; 3*d*, the slight projection which this surface made above the surrounding level, while their adherent surface was in a manner chased and set into the vascular substance as far as the proper membrane; 4*th*, the appearance of the indeterminate texture, without those regular distinct fibres which organic matter presents to the miscroscope after having been treated by dilute nitric acid; and, 5*th*, the influence which it is generally allowed, is exerted on the formation of accidental ossifications in the blood-vessels, by a slow chronic inflammatory action, analogous to that which must have been long proceeding in the walls of the portal vein, in the instance above recorded;—I confess that all these circumstances appear to me to form so many arguments in favour of the pathological theory proposed.

The influence of chronic inflammation in giving rise to calcareous deposits and transformations is admitted by many authorities.

"Ossification of membranes and vessels," says Tommasini, "is also one of the results of inflammation. In order to consider this subject profitably, it is requisite to take a retrospective view of the etiology. It is necessary to consider what the ossified pieces which we meet with in dead bodies have been in the first stages of ossification, and during the first symptoms of the disease, when it may perhaps yet be checked, and be capable of being restrained. Now everything informs me that the condition of incipient or approaching ossification is inflammatory, and that all the causes to which this result most frequently succeeds are of the nature of stimulant agents." Tommasini dell' Inflammazione, Tomo ii. § 218. Pisa, 1827.

In 1810, Professor Francis Aglietti had already published similar ideas relative to what he calls *Lithiasis* of the arteries, as may be seen by perusing the twenty-seventh note of page 298 of the first volume of the Italian translation of the Morbid Anatomy of Dr Baillie, by Dr Paolo Zannini, Venezia, 1819.

In the treatise by Kreysig on Diseases of the Heart, published at Berlin in 1814, that author announces the doctrine that petrefaction and ossification of the heart and of the blood-vessels is merely a deposition of plastic lymph, abounding in calcareous matter, which takes place in consequence of a morbid irritation developed on the internal membrane, and that the said lymph proceeds to induration, because the earthy particles mutually attract each other as soon as the liquid is at rest.

In the memoir which M. Rayer published in 1823, on *Morbid Ossification considered as a termination of the* PHLEGMASIAE, (Archives Generales de Medecine, Tome i. 1823,) that writer regards ossification of the arteries as a termination of inflammation of their fibrous membrane. This doctrine is the same as that professed by Crescimbeni in 1827, in his *Memoria sopra Inflammazione delle systemi arteriose, venose, et limphatice, et nervose*, to which a prize was assigned by the Medico-Chirurgical Society of Bologna. § 17, *Alterazione organiche dell' arterite lenta.*

[This doctrine has indeed been long in different forms familiar to pathologists. It may be said to be presented under two forms; one, that chronic inflammation ends in or gives rise to ossification, as is very manifestly seen in certain instances of pleurisy, pericarditis, endocarditis, peritonitis, or orchehymenitis, and meningitis; the other, that the penetration of certain textures by calcareous matter, or the deposition of calcareous matter upon them, is attended by a process of chronic inflammation. These doctrines differ little in reality; the latter is probably a more strict expression of the matter of fact, without referring to the cause or causes.]—ED.

With regard to the composition of these ossiform lamellæ, the results which I have obtained from them by treating them with dilute nitric acid, joined to the chemical analyses of venous concretions made by Gmelin, John, Prout, Hasse, and Lehmann, show that they contain, besides the organic matter previously mentioned, phosphate of lime, a little carbonate of lime, and a very small proportion of carbonate of magnesia.

§ 5. *Relation between the Symptoms and the Morbid Changes found in the Dead Body.*

The question next to be considered is, whether chronic inflammation of the VENA PORTÆ, and consecutive obliteration of the canal of this vessel, have or have not given origin to the disorder under which the subject of the above given history expired. Was there any necessary dependence or evident relation of causation between these pathological lesions and the functional disorders which the case presented; for instance, askites, infiltration of the lower extremities, chronic sanguinolent diarrhœa, and dull pains in the abdomen; between these same morbid lesions and certain other abnormal circumstances observed on inspecting the dead body; for instance, unusual dilatation and extraordinary distension of the mesenteric veins, the intra-intestinal hemorrhage, the state of injection, hypertrophy, and infiltration of the intestinal walls, and the ulcerated state of the villous membrane?

It is known, and M. Bouillaud has the credit of having first placed this matter out of doubt, that obliteration of the portal vein in general causes askites.* Any obstacle to the free circulation of the blood through this vein, situate, not in the liver left sound, but in the course of this vessel or of its principal abdominal divisions, may induce within the cavity of the peritonæum an unusual accumulation of serous fluid. This result, it is to be observed, may be produced at will, by placing, after the example of Lower, a ligature upon the portal vein in a living animal. The explanation of the result is easily conceived, when we remember that the system of the portal vein, which is independent to a certain extent of the general venous system, is, as to the peritonæum and a great number of organs contained within the cavity of the abdomen, what the crural and hypogastric veins are, as to the lower extremities; and that, consequently, the mechanism of askites is in this instance quite the same as dropsy of the lower extremities.

[The experiment performed by Lower here mentioned, is neither so conclusive nor so free from objection as is commonly believed. Seventy-six years ago,—namely, in 1774,—Hewson, who had repeated the experiment urged, against its validity, the objection, that, as the lymphatics pass so close to the vein, several of them might, or must have been compressed by the vein, and perhaps by the ligature. In the dog on which Hewson* performed the experiment, death took place half an hour after, and very little water was found within the abdomen.

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* Hodgson, Travers, and Beclard record instances which are at the same time favourable and approach to the views of M. Bouillaud upon dropsies, consecutive upon obstruction of the veins. MM. Rayer and Bricheteau enclosed in a ligature the principal venous trunks, without any effusion of serous fluid following. It was the opinion of Bichat, that obliteration of the veins and even of the principal venous trunks, did not in general produce dropsy. (*Corbin de l'obliteration des veines comme cause d'oedeme, &c.*, Archives Generales de Med. Avril 1831.) Morgagni shows that he knew that an impediment to the circulation may give rise to an abnormal accumulation of serous fluid. Epist. lxxxiii. 19, *et seq.*

Lower, who also enclosed in a ligature the jugular veins of a dog, states, that the effect was to render the head of the animal dropsical. Against this experiment, Hewson urges the objection, that these veins have since been repeatedly enclosed in a ligature without œdema of the parts above ensuing;—a result which Hewson ascribes to the free communication of the anastomosing branches by which the circulation interrupted in the main trunk is continued by the collateral circulation.

These, however, are not the only serious objections against the inferences deduced from the experiment of Lower. First, any operation of this kind must be attended by so much local injury, that effusion of serum and sero-albuminous fluid within the cavity of the peritoneum cannot fail to ensue. Whatever serous fluid, therefore, is found within the peritoneal cavity after such an experiment, may be as much regarded as the effect of inflammation, as obstruction to the motion of blood in the vein.

Secondly, the injury done to the vein alone may, by inducing *phlebitis*, be sufficient of itself to cause death. In this case it must be difficult to distinguish between the effects of the pathological state of the vein and the mere mechanical obstruction. In short, the ligature of the vein, however performed, is a most complex injury; and it is quite impossible to separate the mere mechanical effects from those which are physiological and pathological. The experiment, in short, goes for nothing, as to the inferences which are commonly deduced from it.

The only mode in which just conclusions can be drawn from such an experiment is, when it is performed by nature herself; that is, when the portal vein or the *vena cava* is obstructed gradually and independent of inflammation, in consequence of some internal cause. There is strong reason to believe that, even in this instance, serous fluid would be effused at first within the abdomen; and if it were afterwards removed by absorption, that must be in consequence of the collateral circulation undergoing eventually a sufficient degree of development to maintain the venous circulation in a normal condition. On this subject several instructive considerations are given by M. Raikem at the close of § 5.

This result, however, appears to be one which cannot be expected to take place in all instances; and it is probable, that in most instances in which any large vein is obstructed, dropsical effusion, if it do not take place soon, will do so after some time.]—ED.

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* Experimental Inquiries: Part second. Containing a Description of the Lymphatic System, &c. By William Hewson, F. R. S. London, 1774. Chapter ix., p. 143.

Another effect which may be ascribed to obstruction of the portal vein, is the profuse, obstinate and sanguinolent diarrhœa, with which the patient was for several months afflicted. It is indeed natural to think, that from this obstacle to the motion of the blood in this vein, there should result impediments almost insurmountable to the return to the heart of the blood conveyed by the intestinal, mesenteric, splenic, pancreatic, and gastric veins; that this blood must be accumulated in these vessels, distend their walls, become retrocedent,* fill the venous rootlets, stimulate them violently, at length oppose the performance of their absorbing function, and cause in them inflammation, rupture and effusion of their contents. "If," as Sauvages says, with much reason, "the liver be obstructed or much diseased, or if the portal vein have been obstructed, though there be no fever, bloody serous fluid may be effused within the intestines. The blood cannot be impelled into the minute arteries, without distending and causing inflammation in their colourless branches; and hence results inflammation of the intestines, and at the same time extravasation of blood into their interior."†

In instances, also, of obliteration of the trunk of the portal vein, in consequence of inflammation, it has been ascertained that the blood had flowed backwards into all its radical or tributary branches, and that it had distended them and rendered them hypertrophied and varicose; that the spleen was swelled; that hemorrhoidal attacks had ensued, and had in some instances formed considerable tumours; and, lastly, that matters, mucoso-bilious, then blackish, had been brought up by vomiting, and had been discharged by stool, that is, that true intestinal hemorrhages had taken place.

Among the morbid phenomena which accompany chronic *pylephlebitis*, Dr Messow specifies the following: "Internarum venarum tanti est expansio, ut serius ocyas dirumpantur, et vomitum ac diarrhœam massarum cruentarum nigrarumque adducant. Statu fere a febrili obliteratio venæ portarum aut melænam cum hæmatemesi, aut hydropem cum ictero frigidum procreat. Melænam quod attinet, lien polyæmia affectus tumere ac dolere incipit. Dyspepsia, ructu acido alioque tarda laborantes repentinus aggredi vomitus, quo sanguis niger et acidus emittitur. Accedere denique diarrhœa cruenta et hæmorrhagiis hisce modo cedentibus, modo redeuntibus, perire post aliquot tempus aegroti ex anæmia," &c.[*]

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* Infarctus primarium sibi piluiunt sedem in systemate venæ portæ ob rationes, a Stahlio et Frid. Hoffmanno, egregie traditas, quia nimirum ob defectum valvularum, et post perpetratas secretiones sanguinis spissi in hac vena tardior est progressus, &c. Koempf de Infarctu vasoria Ventriculi. Basileæ, 1751.

† Si hepar obstruetum fuerit aut male affectum, vel si vena portarum obstructa fuerit, etiamsi nulla febris adfuerit, serum sanguinolentum potest in intestina delabi. Non potest sanguis trudi in ultimas arteriolas quin illarum ramulos lymphaticos diducat et inflammet; unde phlogosis intestinorum et simul effluxus cruoris in cavum. Nosologia Methodica, Tome ii, p. 325.

Necroscopic researches agreeing in this respect with symptoms, show that inflammation of the portal vein is not always limited to this vessel, and that it may extend, by a species of diffusion, to neighbouring and adjacent parts. Thus, when it is propagated to the venous rootlets, from which arise the veins of the stomach and intestines, it attacks, conformably to the law of continuity, and following, so to speak, a centrifugal direction, the parietes of the veins, and directs its encroachments upon a part of the intestinal tube. In such instances, we find the mucous membrane of the stomach softened, and becoming detached from the other membranes a great deal more easily than usual. In other instances, it is the intestinal mucous membrane which presents a remarkable degree of softening to an extent more or less considerable; it is then discoloured, or hyperemiated, or hypertrophied, or infiltrated, or ulcerated, as I had occasion to see in the instance of the patient Loxhay. In the instance of suppurative *pylephlebitis*, which was observed by M. Schoenlein at the Hospital of Wurzbourg, inspection showed that the stomach was injected and much softened; the jejunum and ileum were hyperemiated, of blackish-red colour, and their interior filled with blood, and their mucous membrane emphysematous, very much thickened, and traversed by vessels much enlarged and distended with blood.[†]

I think that it is impossible to doubt that the mechanical stagnation of the blood exerts a great influence on the production of these secondary forms of inflammation; nevertheless, the mode of propagation of inflammation of the portal vein, which is regarded as most common, is that which takes place by continuity of tissue. This may be accomplished in two modes.

1. Phlebitis extends not only along the normal direction of the course of the blood from the rootlets, or the branches towards the venous trunks. It may also, as Abernethy has shown, and as I satisfied myself in 1835, extend in the opposite direction. As to inflammation of the portal vein, we have seen it on the one side propagating itself to the splenic vein, the pancreatic vein, and the intestinal veins, and, on the other, extending to the hepatic veins, the *vena cava*, the coronary veins, the pulmonary arteries, and even the lungs, and especially that on the right side. This is established by the circumstances of the cases quoted and recorded by MM. Dance, Reynaud, Duplay, Lambron, Meckel,

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[*] *Pylephlebitis*, inflammation of the portal vein, from Πυλη, *porta*, and Φλεβς, *vena*.

[†] Baczynski, p. 29, *et seq.*

Andral, Bailling, Mohr, Schoenlein, as well as the works already mentioned.

2. Nevertheless, it occasionally happens, that inflammation of the system of the portal vein proceeds from the capillary vessels in which it has originated, and extends successively to the larger branches, and even to the trunk of the vessel. This diffusion ought not to be matter of astonishment. In general, phlegmasial attacks, which affect the different organs of the economy are disposed to pass from the capillary vessels primarily affected to the venous trunks to which they belong. M. Ribes quotes an instance of *phlebitis* in the arm, which had succeeded to frost-bitten sores on the hands. MM. Cruveilhier, Dance, Fizeau, and Lambron, relate each an instance of hepatic phlebitis consecutive on an attack of inflammation in the intestinal tube and in the biliary ducts. MM. Andral, Bouillaud, Mohr, and Schoenlein, have in like manner witnessed analogous cases. To these I add the following from the work of Tommasini.

A woman, 30 years of age, between the third and fourth month of pregnancy, was attacked by gastric fever, accompanied with headach and sleeplessness. Blood-lettings, local and general, were employed; purgatives and saline antiphlogistic drinks were administered; and, under the use of these measures, about the sixth day there ensued a degree of amendment so considerable, that it was expected that convalescence was at hand. At this period, however, the patient felt all at once, in the right thigh, a very acute pain, which extended upwards to the region of the kidneys, and which was soon followed by abortion taking place easily and without accident. The continuance of the pain in the thigh led to the belief that the crural vein was inflamed. The suffering very soon extended to the whole member, which became swelled, stiff, and tense; the superficial veins were of a violet colour. On the seventeenth day of the disease, the pulse was small and very rapid; shiverings were frequent; there was excessive prostration of the physical and moral powers; a sense of fainting, and anxious respiration. Notwithstanding the treatment employed, and the administration of extract of squill, which Tommasini has often found useful in acute phlebitis, the state of the patient rapidly deteriorated, and she died on the eighteenth day of the disease, forty-eight hours after the time at which the pain in the thigh had first evinced its presence.

Upon inspecting the body, the ovaries were found slightly swelled, and sprinkled with some points in a state of suppuration. The trunk of the crural vein on the right side and all its afferent branches, the internal and external iliac veins, the abdominal and thoracic *vena cava*, the portal vein, and the pulmonary veins, were very much inflamed, though the observer recognised in them neither purulent matter, nor clots, nor fibrinous concretions, nor pseudo-membranous exudations. The marks of inflammation of the venous system were much more strongly displayed, as examination approached the thigh, where the crural vein presented a livid blackish colour, like that which is observed in the soft parts, which are the seat of an incipient gangrenous degeneration. The arterial system partook a little in the state of phlogosis.*

In chronic gastritis, it is not uncommon to find beneath the softened mucous membrane veins very much enlarged, and presenting evident marks of inflammation. In a peasant, aged 70 years, who had died suddenly, and who had laboured for some time before his death under symptoms of chronic gastritis, accompanied by fits of vomiting, I found, on inspecting the body, hypertrophy of the right side of the heart, well advanced softening of the aorta, and of the hepatic parenchyma, and hydrothorax. The stomach was hypertrophied and larger than usual; its inner surface was covered by thick whitish mucus; and, for the space of from three to four inches, the coronary vein of the stomach, which winds along the large curvature, was dilated to so great a degree as to be three lines in diameter. The walls of this vein were hard, thickened, of an intense red colour; and its cavity was filled and obliterated by a reddish clot, very firm, and which adhered to its internal surface.

In 1825, I attended professionally a butcher of Volterra, aged 38 years, who was labouring under an attack of severe continued fever, which I regarded as connected with gastro-enteritis, associated with inflammation of the spleen. He died on the twentieth day of the disease. On inspecting the body I ascertained the existence of violent inflammation of the mucous membrane of the stomach and small intestines, intra-intestinal hemorrhage, unusual softening of the spleen, and manifest traces of an attack of phlegmasia, which extended to the *vena portæ*, to its hepatic ramifications, to the aorta, and even to the endocardium, which lines the auricles and covers the mitral and tricuspid valves.†

Inspection of the bodies of persons cut off by hemorrhages from the stomach has several times made known an obstacle to the circulation of the blood, situate in the trunk of the portal vein; and, among the pathological alterations which have become manifest in this class of cases, authors have noticed a state of varicose dilatation of the *vasa brevia*, and of the intestinal, mesenteric, and mesocolic veins.

Physicians have long endeavoured to explain the production of hæmatemesis, of intestinal hemorrhagy, and especially that of hemorrhoids, by the difficulty which the blood contained in the portal vein and its different branches encounters in traversing the liver and its vascular system.

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* Tommasini della Inflammazione, Tomo iii. § 338, p. 583. Pisa, 1841.

† The history of this case is recorded in the Repertoire Generale d'Anatomie et de Physiologie Pathologique, published by M. Breschet.

A young girl, much emaciated, and in a state of great weakness, had been suffering for two months under considerable diarrhœa, to which were then added numerous dark coloured alvine discharges; intense fever; acute pain at the epigastric region, and vomiting about half a pound of blood. She died the day after her admission to the hospital.

On inspecting the body, the following state of organs was found. Behind the stomach was a tumour the size of the fist, formed by the agglomeration of crude dense tubercles. The portal vein, subjacent to this tubercular mass, was obstructed by a clot of reddish blood, lamellated and feebly adherent to the internal membrane. This vessel, nevertheless, was free within its sheath, which did not partake in the tubercular induration. Below this point the clot became more adherent, penetrating especially into the superior mesenteric vein; it was colourless and greyish. The internal coat of the vein was of a slightly violet red colour, rugous in certain points, and in those points it was with difficulty separated from the clot which obstructed the interior of the vessel. The external tunic of this vein was injected, thickened, of a violet red colour for the space of one inch and a half in the portion of the vessel to which the fibrinous clot adhered most firmly. All the veins which go to the large mesenteric vein, and the other affluent veins of the *vena portæ*, below this last vessel, were distended and filled with black half-coagulated blood. The gastric arteries, the aorta, and the chambers of the heart were empty.[†]

It is equally certain that the hemorrhoidal flux, as well as congestion of blood in the arterial capillaries of the stomach, and intra-intestinal hemorrhagy, may arise from diseases which stop and prevent the circulation in the system of the portal vein; for instance, obstructions, indurations, abscesses, encephaloid cancers, and other tumours of the liver and of the mesentery.

When inflammation is fixed for some time on the *parietes* of the veins, these may be attacked by ulceration, perforation, or softening; they may be dissolved and disappear; and even, in some instances, they burst during life, as happens in the instance of the *vena portae*. Instances of this last accident are recorded by Vesalius, Schenke, Testa, and Meli.

First, however, I mention the following facts, illustrative of softening and suppuration:—

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[*] J. Koempf *De Infarctu Vasorum Ventriculi*; especially the new edition of the work quoted by Puchelt.

[†] Instance of Entero-hæmorrhagy from obstruction of the *Vena Portæ*. Gendrin, Traité Philosophique de Medecine Pratique, T. i., p. 287.

Upon inspecting the body, in the instance already referred to, of the person who died in consequence of inflammation in the portal vein, at the same time suppurative and adhesive, M. Schoenlein found the inferior cava considerably dilated, and presenting, for the space of about two inches, an intense red colour, and a degree of softening of its membranes which had proceeded so far, that the slightest pressure was sufficient to lacerate them.*

In 1809, I inspected, at the Hospital St Antoine at Paris, the body of a young man who had died in consequence of inflammation of the inferior *vena cava*, and of several branches of this vessel; and, nowithstanding the most minute researches, I could discover no trace of the crural vein of the right side. Its course was occupied by a line of purulent matter perfectly circumscribed in the course of the vessel. This tract of suppuration was continued to the ham. The inference is that the vein was dissolved in suppuration.

The following are examples of laceration:—

"Bellarmati, a distinguished lawyer," says Vesalius, "at Sienna, addressed me in the shop of a bookseller at Pisa, and made some inquiries relative to the state of his health, which for several years had been indifferent. As in the course of conversation I made mention of obstruction of the passages of the liver and gall-bladder, and also of the spleen, he stated that he would come the following day to the anatomical theatre, and examine carefully those organs, the structure of which came in ordinary course to be explained. When he had gone home, and occupied himself for some hours with study, and presently after began to take supper, he complained of remarkable weakness of body and embarrassed breathing; and, after the administration of some remedies which were thought suitable for a discharge of bile into the stomach, and which had been long familiarly used by his physicians, he expired."

"When I dissected and examined the body, I found immediately that the whole of the blood, still warm, had flowed into the cavity of the peritoneum, and formed an accumulation like that species of dropsy, which is named *askites*. The occasion of this discharge of blood had been a species of indurated abscess in the extremity of the *vena portae*, which, having undergone suppuration at a certain point, and burst, had allowed the blood to escape. We saw, accordingly, the liver perfectly pale, the surface not smooth, but very irregular, and roughened with numerous smooth tubercles. In the concave part of the liver (the sinus), the whole course of the portal vein was distinctly seen; the substance of the liver having been completely retracted and wasted at the inferior seat of its vessels."

"The anterior part of the liver (the convex portion), and the

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* Baczynski, *loco citato.*

whole left tobe, was rendered as hard as a stone; but the posterior portion, where is situate the trunk of the *vena cava*, was very soft, and seemed to be corrupted by gangrene."

"The gall bladder was unusually pale, and contained eight small gall-stones."

"The spleen was very large, extended forwards, soft, and something whiter than natural."*

A prince, aged fifty-four years, corpulent, tormented for a long time by violent pains, nephritic and gastralgic, and in whom the general strength was much enfeebled, fell into a state of swooning, lasting for several days. He expired in a fit of fainting. On inspecting the body, the stomach was found half-filled with coagulated blood, and a breach of continuity, with laceration of the portal vein, was observed at the concave surface of the liver.†

Nothing is more common, says Testa, than inflammatory attacks in the *vena azygos*, and its intercostal branches, in persons labouring under peripneumomy. The instances of inflammation of the portal vein are in like manner not very uncommon; and, in illustration of this fact, he quotes from Folchi, former professor at the University of Ferrara, the following case.

After a supper party, at which she had been very gay, a young girl, in whom the catamenia had been disordered during the previous month, felt all at once sensations of lassitude, weariness, giddiness, and had vomiting. Her features were very much changed; and she felt great anxiety, and a sensation of constriction at the precordial region. The pulse, which was scarcely perceptible, became slower and slower every instant, then was intermittent, and at length ceased entirely. At the same time her strength departed, the surface was covered with cold sweats, and death took place at the end of several hours of this disorder.

When the body was examined, the abdominal cavity was found to be filled with bloody serum, and the abdominal viscera were covered with large clots of blood. In the region of the liver a larger quantity of blood of a darker colour denoted that the fatal origin of the sudden death was situate in that part. At the point where the *vena portæ* enters the concave surface of the liver, I found that vessel relaxed, and varicose; and not far from its entrance, a small wound opening in the long direction of the vein for the space of about half an inch. The other parts were sound.‡

Upon examining the body of a person who died under symptoms of bilious fever, and whose body presented the anatomical characters belonging to inflammation of the portal vein, M. Meli found, a little on this side of the transverse furrow of the liver, at the point where this vein enters the gland, a longitudinal rent with fringed margins, about eight lines long; and the membranes of this vessel were softened to so great a degree, that they were reduced to pulp under the fingers.

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* Andreæ Vesalii Epistola de Radice Chinæ Opera Omnia. Lug. Bat., 1725. Vol. ii., p. 674.

† Schenkius Observat. Med. Rariores.

‡ Testa Delle Malatti della cuore, 18.

The same author further records an instance in which he found the portal vein distended by blackish clots, and having its walls thickened, and of deep red colour. The internal surface of this vein was lined by a layer of plastic lymph, dense, tenacious, flocculent, irregular at its surface, and analogous to the epichorion. Between this layer, and the surface above mentioned, was found a matter of greater existence, greenish-yellow in colour, and which resembled purulent matter. At this point the membranes of the venous trunk possessed little resisting power, and were easily rent.*

In certain favourable circumstances as to age, temperament, and other conditions, and when the obstacle to the current of blood in the portal vein is formed in a manner sufficiently gradual, in order that the anastomosing branches may have had the means and time to be enlarged to such extent as to be able to perform the part of the circulation, interrupted in the principal trunk, we conceive that life, and perhaps even health, may be, to a certain extent, compatible with this abnormal condition. Though this must be regarded as a wonderful operation of conservative nature, yet in its course it ought to be favoured by all the means of art. Agreeably to this manner of viewing the subject, askites ought not to be considered as necessarily ensuing on obliteration of the portal vein, because the serum, which its absorbing rootlets draw from the cavity of the peritonæum, would find, besides those passages which the lymphatic vessels present in the anastomotic branches, which become unusually large, other channels to be conveyed into the abdominal *cava*, with the blood furnished by the intestinal, mesenteric, and mesocolic veins. In a work, published a few years ago, M. Corbin quotes several instances of obliteration of venous trunks effected spontaneously, or by means of ligature, and which were neither accompanied nor followed by œdema in the corresponding members. An illustrative complementary fact deserving notice is that M. Fauconneau-Dufresne further assures us, that the coincidence of development of the collateral circulation, with diminution of dropsy of the abdomen, has been sometimes observed in instances of obliteration of the portal vein.

There are physicians who apprehend that death may be the consequence of the abdominal blood not depurated by the liver being admitted into the general system. This apprehension, nevertheless, is rather inspired *à priori*, by preconceived physiological ideas, than founded upon carefully observed facts and conclusive experiments. It may be asked, if it is proved that those individuals in whom the portal vein opened directly into the inferior *vena cava*, instead of proceeding to the liver, and in whom this organ continued to secrete bile, as it continued to do when the portal vein was obliterated, as was the case in the subject of my observation;—is it proved, it may be asked, that these individuals have died, or have experienced the peculiar functional disorders dependent on a bilious intoxication or diathesis, proceeding from a congenital anomaly? On the other hand, it is not to be forgotten that this arrangement is not an anomaly in the MOLLUSCA, in which the blood goes to the liver only by a branch of the aorta?

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* Meli Sulle Febbri Biliose, 1822. Lobstein traité d' Anatomie Pathologique, T. ii., § 1127.

§ 6. *Connection between Intermittent Fevers and Inflammation of the* VENA PORTAE.

Bichat said forty-four years ago, that the tissue of the veins is often inflamed; and since that time numerous facts have confirmed the accuracy of this opinion. Contemporary authors of high merit go so far as to assign to *phlebitis* an important part in pathology. Thus, some place in the venous capillary system, the immediate seat of inflammation, and of all the alterations in organic composition, among whom may be mentioned Rasori, Ribes, Cruveilhier. According to others, again, as Armstrong, Breschet, Meli, Mendini, Benvenuti, &c., bilious fever, typhus, typhoid fever, ardent fever, or *Causus* of the ancients, intermittent fevers, and pernicious fevers, are only different forms of *phlebitis.*[*]

Is it to chronic inflammation of the portal vein that ought to be ascribed the accession of obstinate intermittent fever, under which the nailsmith, described in the history above given, had suffered at the period of his residence in Zealand? Facts are wanting to warrant me in giving an opinion on this point.

It appears, nevertheless, to result from researches made in the persons of individuals cut off by intermittent and pernicious fevers,[†] that the venous system of the abdomen, the spleen, the liver, and the right side of the heart, are usually the seat of alterations more or less severe; that it is not uncommon to find marks of inflammation in the abdominal venous system; but that we find more frequently a species of excentric hypertrophy, or extraordinary varicose condition; and, lastly, that the lesion of which the organs now mentioned are the seat, affects particularly nutrition, and that the black blood contained in these vessels is evidently altered in *crasis*. (Senac, Lieutaud, Bally, Gendrin, Trousseau, Piorry, Speranza, Puccinotti.)*

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[*] This is either not very intelligible or not quite correct. Armstrong did not say that fevers consisted in venous inflammation. All that he contended for was, that venous congestion was a very common state in fever, and gave rise to many of the phenomena and effects observed in fever. Venous congestion is congestion of the veins with blood, not inflammation of these veins. As to the other authors to whom these doctrines are ascribed, they must answer for themselves.—ED.

[†] See the learned Memoir of Dr Benvenuti of Padua, on the Anatomy, Physiology, and Pathology of the Venous System, published at Milan in 1840.

Dr Mendini has published two memoirs, in which he endeavours to prove, that the proximate cause of intermittent fevers is seated in the system of the VENA PORTÆ, and consists in a hypersthenic state, with distension and phlegmasia of this vein. According to this author, no fever actually belonging to the class of periodical disorders can exist without receiving the impression or the form, and at the same time the essential nature, of a hypersthenic condition of the portal vein. The more free a fever patient in a state of apyrexia is from the marks and symptoms of this disorder, the more decidedly Mendini believes himself authorised in forming the conclusion that the ague proceeds from an attack of *sub-portitis*,† or from an attack of *portitis*, primary, secondary, or compound. To consider regular intermittent fevers as depending on any other organ than the Portal Vein, would be, in his opinion, as if it were proposed to represent that continued fever can exist without lesion in the cardiaco-arterial system, or mania in a person without a brain; so characteristic he thinks the type, the constitution, and the physiognomy of intermittent fever in numerous respects to be.

I shall not enter into those details which would be required by a critical analysis of the theoretical ideas of Mendini. But I feel it necessary to say, that these ideas do not accord with the practical facts which I have had opportunities of collecting on Intermittent Fevers and Pernicious Fevers, during a residence of twenty years in Italy, not far from the Tuscan Maremme. In all my necroscopic researches, made with the greatest care and without preconceived ideas, I have in general found, in patients cut off under intermittent fevers, whether in the town or in public establishments, or in the hospital, the service of which was entrusted to me, that the general venous system, and that of the *vena portæ*, contained only a small quantity of blood, usually more or less serous, poor in fibrine and in red globules, and that there were few or no fibrinous concretions in the large venous trunks, and in the right cavities of the heart.

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* According to Signor Giacomini, pernicious fever is, on the other hand, only a very intense erysipelatous *arteritis*, with a very deep-set nucleus, or with congestion in some organ essential to life. I have found only in two instances the internal surface of the aorta coloured red in the bodies of persons dead of pernicious intermittent fever. There was no trace of arborescent hyperemia, or of any other phlegmasial character in the subjacent tunics.

† By *sub-portitis* is meant inflammation of the portal vein. The name is not very happy, and is hybrid, being Latin with a Greek termination. The name given by Messow, of *Pyle-phlebitis*, would be more correct.—ED.

In one single instance, in the person of a young Lombard, aged twenty-five years, who died at the hospital during the third septenary period of a continued pyrexia, accompanied with an eruption on the skin of many suppurating pustules, I found the internal surface of the *venæ cavæ*, and of the portal vein, of a very intense red colour, and which unwonted coloration was in like manner presented by the mucous membrane of the air-passages. The arterial system presented no perceptible alteration; and the intestinal mucous membrane alone was slightly injected. I remarked further, that decomposition of the dead body had taken place much more rapidly than could have been expected, considering the time which had elapsed after death, the species of death, the season, the age and the constitution of the individual.

The Anatomico-Pathological Treatise on Intermittent Fevers, published by M. Bally of Blois, contains no example of inspection in which there is evidence of inflammation of the portal vein. It is the same with the work of Dr Folchi, first physician to the Hospital of the Holy Ghost at Rome, published in 1840, and which contains a great number of pathological histories of cases of Pernicious Fever. The researches of this practitioner agree with the results of my own observations. The organs in which Dr Folchi most frequently observed marks of hyperemia and inflammation are,—the brain, and its membranous coverings; and the gastro-intestinal mucous membrane. A form of well-marked softening of the spleen he observed in all the instances. This lesion is also the only one which I have uniformly observed in the numerous inspections which I have made.*

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* I may here observe that it is conformable to the general results of many facts, to say, that we rarely find the spleen without change in the bodies of those patients who have died of intermittent fevers in the marshy countries of Italy.

Some physicians, to whose opinions I willingly adhere, admit with Senac, F. Hoffmann, and Borsieri, that regular or legitimate miasmatic intermittent fevers, which yield to the employment of one single medicine, depend on one single specific cause, inhaled or conveyed into the circulating current, varying according to its degree of action, and according to the organ in which it fixes its particular seat; and that this cause, when it attacks not directly the brain, affects the nervous system of the spinal chord, and of the trisplanchnic nerve, the ramifications of which then propagate the disturbance and disorder in the functions of the organs of circulation and digestion. Among the practitioners of whom I speak, are some, who, in support of the justice of their opinion, invoke the aid of the results of necroscopic researches, which show often proofs of the existence of phlegmasial attacks in the persons of individuals dead of pernicious intermittent fevers. I can, nevertheless, affirm, that we do not always observe in these instances, manifest indications of inflammation in the organs. Puccinotti even alleges that in certain spots, and during certain seasons, and especially in 1820, he observed little or no phlogosis connected with the pernicious fevers of Rome. From these facts it

Such are the reasons derived for the most part from pathological anatomy, which induce me not to consider inflammation of the portal vein as the proximate cause of intermittent fevers and pernicious agues. I am nevertheless persuaded, that the fever which in general accompanies certain periods of *phlebitis*, and of pylephlebitis in particular, assumes occasionally semeiological characters, which differ so little from those ascribed to true intermittent or miasmatic fever, that it is not in all instances easy to distinguish these affections from each other.

In *phlebitis* which terminates in suppuration, the purulent matter is sometimes mingled with the blood, and enters the current of circulation, and in this instance there are speedily manifested disorders which it is impossible not to ascribe to a cause of this nature.

Most usually after violent and prolonged fits of shivering, which are sometimes repeated with a certain periodical regularity, as in the case of intermittent fevers, there arise all at once an assemblage of severe symptoms, which appear at the same time to proceed from the principal sources of life, and to denote the presence of a deleterious influence acting on the organism. (DANCE).

When inflammation of the *vena portæ* terminates in suppuration, the febrile symptoms which ensue vary according as the disease is acute, slow, or chronic.* In acute suppurative pylephlebitis, with the intense fever which the patient presents, are associated all at once shiverings, the duration of which is from about ten to fifteen minutes. The type of these shivering fits is never regular, and they are manifested once or oftener in the course of twenty-four hours. These shiverings are in some instances very frequent; in others very rare; sometimes they disappear for the space of one or two weeks, though the fever is not in the meantime rendered milder, the condition of the patient on the contrary becomes progressively worse;† and this fever, which presented at first the characters of an acute febrile disorder analogous to the *Causus* of the ancient physicians, to a gastric or a bilious fever, assumes the typhoid type.‡

On the other hand, if inflammation and suppuration advance slowly and in a chronic manner, the fever assumes the hectic and not the typhoid type; but this fever does not in general pursue a chronic course.§ The fits of shivering, by which it is accompanied, return in some instances with so great regularity, that they cause the disorder to be taken for an intermittent fever. I

results, that it is sufficiently probable, that inflammation, which in its nature is never intermittent, may accompany or complicate miasmatic intermittent fever, but that it constitutes neither the source nor the proximate cause of that disorder.

* Baczynski, Kaether, Sander, and Messow.

† Case by M. Schoenlein.

‡ Dance.

§ Case by Mohr, and the monographies by Messow and Sander on Inflammation of the *Vena Portæ*.

may say, in order that the observer may not be misled on this point, that these shivering fits are ordinarily of short duration; they are rather chills (*horripilutiones*) than true shivering fits, and they are constantly followed by continuous burning heat. Besides these, diarrhœa and colliquative sweats, as well as aphthous complaints, take place.

Lastly, the paroxysms of hectic fever, which, at the beginning of the attack often present the tertian type, afterwards assume the single or double quotidian type.*

When the particular shiverings which are manifested during the attendant fever of suppurative pylephlebitis are frequent, they may easily lead the observer to believe, that he beholds a complication of continued fever with intermittent fever; but they ordinarily affect not a regular type, and when they appear to follow this type, there are always other symptoms which ought to make the observer reject all idea of intermittent fever, true and erratic.

Hectic fever accompanied with repeated shiverings being nothing else but the result of general infection of the blood, by the purulent matter formed and present in the inflamed vein, differs further from intermittent fever, whatever be its type, in this circumstance, that no relation exists between the cold stage and the hot stage.† Thus, when we examine attentively the cold and hot stages of intermittent fever, their duration, their intensity, and other characters, we find most commonly a correspondence between them in different points of view. In hectic fever, on the other hand, the stages have no correlation. The period of cold is very short, and it consists less in shiverings than in chills, which are followed by violent heat of long duration. Further, it is ordinarily in the morning, before noon, or in the course of the day, that accessions of intermittent fever manifest their presence; while it is towards evening, or the approach of night, that patients attacked with suppurative pylephlebitis most frequently experience the chill sensations of which I have now spoken.‡ Lastly, this disease presents, as has been already stated, in its progress, local symptoms, pain, aphthæ, fits of vomiting, diarrhœa, and other morbid phenomena, which are usually foreign from intermittent fever.

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* Queribundi, subito in febrem incidunt, quæ febris sive gastrica sive biliosa non ita multo post in hecticam abit, et horroribus quidem interdum tam regulariter redeuntibus, ut hecticam pro intermittente habere possis. At vero brevem horrorem vel potius horripilationem calor excipit ardens et longum per tempus constans. Præterea accedunt tubi intestinalis cutisque colliquationes, et aphtharum eruptiones. Hectica autem tertianæ typum exhibens, typum amplectitur quotidianæ sive duplicis.—Messow, loc. cit p. 42 et 43. See also case by Mohr..

† J. P. Frank, Epitome de curand. hom. morb. Schoenlein.

‡ Mox accedunt symptomata exilis ac tenuis reactionis febrilis. Sæpius vesperi horripilatio ægrotos perfundit, una mutua cum calore, cutis sicca, calidior, minima fervida, pulsus modo acceleratus et mollis, modo tardus debilisque. Baczynski de Vena Portarum Inflammatione. Species Chronica, § 23.

The characters now specified are those, which the observers whose works are known to me, mention as the most decided for enabling physicians to distinguish between the febrile accessions which accompany suppurative pylephlebitis and those which attend intermittent fever.

§ 7. *On the employment of Quinquina in the Symptomatic or Secondary Fever of Intermittent appearance, which accompanies Inflammation of the* VENA PORTÆ, *terminating in Suppuration.*

Of late years, physicians who have had occasion to observe chronic suppurative inflammation of the Portal Vein, accompanied with febrile paroxysms, imitating accessions of Intermittent Fever, have employed the salts of quinine in order to overcome the apparent periodical disposition of this disorder. I know no instance of the disease, in which this therapeutic method has been attended with success, while there are several in which it has been followed by an aggravation of the symptoms of the disease. In vain did M. Schoenlein prescribe muriated quinine to the young sadler, whose disease he observed with great attention at the Hospital de la Charité at Berlin, and in whose case he formed a diagnosis so accurate on the seat and nature of the disease, about two months before the death of the patient. "Impedire non potest, ne febris hectica in dies magis proveniat. Horripilationes enim quotidie, et dein bis diebus singulis intrant, easque excipit ardor et rubor genarum circumscriptus, ac sudor copiosus, sine levamine."

The day-labourer suffering under phlebitis of the portal vein, whose history was published in 1842 by M. Ernest Lambron, also took without any advantage sulphate of quinine, which was administered for well-marked febrile exacerbations, which imitated accessions of ague. The same result took place in another instance of which the same physician speaks. This was the case of a drayman who had an attack of suppurative phlebitis of one of the subhepatic veins, and to whom sulphate of quinine was administered, in order to subdue febrile accessions, in which the three stages, cold, hot, and sweating, were very well marked.

Further, it is my belief that all fevers in which the paroxysms possess a species of periodical character cannot be subdued by means of the preparations of quinine, in the manner in which true idiopathic, specific or miasmatic intermittent fevers are cured by that agent. The employment of these preparations becomes even in certain circumstances detrimental. Intermittent fevers which are false, illegitimate, symptomatic, or secondary, which Rubini calls *Irritative*,* are in some instances connected with the presence of indigestible articles in the first passages, with entozoa in them, as well as the presence of large wounds, extensive suppurating surfaces, purulent collections, skirrhous and cancerous tumours, tubercular disorders, and many other chronic lesions, and particularly with those of the liver, the spleen, the kidneys, the bladder, and similar organs.

Upon this secondary or symptomatic intermittent fever, Borsieri expresses himself in the following terms. " If intermittent fever be known to be secondary or symptomatic, then neglecting Peruvian bark as ineffectual and inadequate to remove the fever, though marked by intermissions, the physician should have recourse to those remedies which are suited to the primary disease."† Similar also is the advice of John Peter Frank upon this important point in practical medicine. " Hence it becomes requisite to investigate the nature of the unknown impediment to cure, which is concealed in obstruction, skirshus, cancer, or similar lesion of some internal organ, especially the liver or spleen. Such lesions demand a peculiar mode of treatment; sometimes admit of none, and are aggravated by the bark improperly administered."‡ In truth, if some febrile paroxysms appear, in certain instances, to be wanting during the administration of quinquina, there may be reason to fear that this amendment is only apparent and of short duration; the form of the disease alone has undergone change often without any advantage, and sometimes even to the detriment of the patient, as has been justly remarked by Van Swieten in certain instances of intermittent hectic fever. " Quae si cortice Peruviani tollatur, pessime se habent tales aegri, et summam patiuntur anxietatem adeoque, si vel minime suspicio collecti alicubi purisadsit, a corticis usu abstinendum omnino est."§

Remembering that certain Intermittent Fevers which resist the employment of sulphate of quinine are associated with an organic disease of the liver, M. Andral refers to Dr Portal as one of the authors who have most strongly dwelt on this fact.‖ This experienced practitioner makes the remark, that intermittent fevers, which depend on this cause, never yield to quinquina, and that this medicine renders them more obstinate, and aggravates the pathological condition on which they depend. In short, this doctrine, which is also my own, in so far as I have verified its correctness in practice, has lately received additional confirmation from another French physician, namely, M. Pidoux. This physician states that he has frequently observed, that the intermittent and periodic affections connected with an immovable organic lesion, or with the constant presence of a material cause, are capable of being affected, modified, suppressed, "for one instant, in short, deranged by sulphate of quinine; but that, in such circumstances, the practitioner obtains not from the remedy any decided curative effects. Too often even it happens, that, in different instances, it exerts no action whatever, and that its virtue can give it no control over the energy of the cause which determines the symptoms.*

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* Memoire sur la meilleure maniere d'empecher la recidive des Fevres periodiques. Couronné in 1805 par la Société Italienne des Sciences.

† Institutiones Medicinae Practicae, Vol. i. § 109.

‡ Epitome de curandis Hominum Morbis, Lib. i. § 48,

§ Commentarii in Aphorismos Boerhaave.

‖ Clinique Medicale, Tome iv.

ART. VII.—*Interesting Cases occurring in the Practice of* JAMES STARK, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Edinburgh.

I.—*Case of severe Gastric Affection produced by a dislocated rib.*

ON the first week of January 1850, during the prevalence of the frost, Helen M'G., about 25 years of age, slipped on the ice, and, in making a violent effort to save herself, felt something give way with a jerk in her right side. She shortly after this got sick and vomited, which gave considerable relief. Whenever she ate a few mouthfuls the sickness returned; while uneasiness, amounting to pain, arose in the region of the stomach, and increased until the food was rejected. Day by day these symptoms increased, so that, after about a week from the accident, more or less uneasiness or pain in the stomach was almost constantly present, and both it and the nausea were greatly increased by the pressure of the stays, which she habitually wore so tight as to deprive the body of all freedom of motion. The stomach now rejected all food within from one to two hours after being taken, the pain and flatulent distension increasing till vomiting occurred. Believing her complaints to arise from the presence of lumbrici, from which she had previously suffered, she took various doses of calomel, turpentine with castor oil, and other laxatives, without bringing away any of these parasites, or affording her the slightest relief. Opium was also taken for several nights without inducing sleep, or relieving the nausea or pain. I was, therefore, asked to visit her on the 19th of January.

Her countenance had the anxious contracted appearance of a person suffering acute pain. The skin felt harsh and dry, but was of the natural temperature; the eyes were heavy and slightly injected; the tongue, which had a somewhat swollen and glassy aspect, was covered with a thick yellowish-white crust, having a blackish stripe down the centre. She suffered from headach, anorexia, occasional tendency to rigors, sleeplessness, and felt weak and dejected. She had considerable thirst, with dry stretched sensation in fauces. The pulse was small and weak, but frequent, being 110 in the minute. She had almost constant sickness and gnawing uneasiness in the epigastric and præcordial regions, and, when she took food, the uneasiness increased till it was rejected by vomiting. The sickness and pain were not perceptibly increased by pressure over the region of the stomach, but pressure over the præcordial region, and especially pressure over the lowest ribs of the right side, immediately increased the nausea and pain. Being induced from this to examine more particularly the hepatic region, it was found that the lowest false rib of that side was dislocated at its sternal extremity from its sternocostal cartilaginous attachment, and that the bad habit she had contracted of lacing her stays tight had so altered the position of this rib, that its now free point projected somewhat inwards and backwards, and irritated the abdominal viscera. Whenever, therefore, the stomach was distended by flatulence, or the point of the rib was by tight lacing brought to press more strongly against it, nausea, pain, and vomiting were induced.

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* Etudes et Observations pour servir a la nosologie des Fievres Intermittentes. Journal des Connaissances Medico-Chirurgicales. Octobre 1843.

The cause of the whole of the symptoms appeared very evident. It appeared to be a case of subacute inflammation of the mucous membrane of the stomach, induced by the irritation and pressure of the dislocated sternal extremity of the lowest right rib. Taking this view of the case, the directions given were, to keep in bed, to avoid all bandages to the waist till the viscera should become accustomed to the new position of the rib, or its point should contract new adhesions; to apply over the præcordial and episgastric regions an occasional mustard poultice or hot fomentation; to take food in exceedingly small quantities at a time, and regulate the bowels by gentle laxatives. Having in similar cases found the best effects result from the use of sulphate of magnesia with sulphuric acid, this was the only laxative employed.

By strictly following these directions the unpleasant symptoms had greatly disappeared by the 29th of January. She had kept strictly in bed, excepting one day, when some friends visited her, when she rose, dressed, and sat with them for about an hour, but the pain and sickness returning, she was obliged to leave them and go to bed. Almost immediately on undressing the pain left the epigastrium, and the nausea shortly after subsided.

By the 8th of February she was able to sit up most of the day, and even assist at light work, provided she did not tighten her stays or apply any bandage to the waist. She could also eat a moderate quantity of solid food without nausea or vomiting being induced. Her tongue was now quite healthy, and her countenance had lost all its anxious expression. At this date, 23d February, she continues well, but still experiences the tendency to slight uneasiness and nausea whenever she attempts to tight-lace her stays, or ties any bandage tight round the waist. The rib remains floating, but from not having been bandaged for some time in the unnatural position in which it was formerly kept, it has risen more to the level of the others, and does not press so much against the abdominal viscera with its free or sternal extremity as at first, and I have no doubt will soon give no further trouble.

This case is curious, as showing how symptoms very analogous to, if not identical with, subacute inflammation of the mucous membrane of the stomach may be produced by the pressure of the sternal extremity of a dislocated rib against the abdominal viscera, when the rib has been forced to assume an unnatural position from tight lacing. That the case was really one of subacute inflammation of the mucous membrane of the stomach seemed to admit of no doubt. There was the peculiar appearance of the tongue, the sense of dryness and pain in fauces when attempting to swallow, the sickness, the vomiting, the pain or uneasiness in epigastrium, the anxious contracted countenance, and quick small pulse, all of which attend and characterize that affection. That these symptoms originated from and were dependent on the irritation of the point of the rib, appeared also manifest from their appearing immediately after the accident, from the nausea and pain being greatly aggravated by pressure on the dislocated rib, whether that pressure was produced by the hand or by tight lacing of the stays. As to the internal remedies employed, none gave half so much relief as the acidulated sulphate of magnesia solution. Even opium itself, which had been taken pretty freely before I visited her, failed to relieve either the nausea, restlessness, or pain. The mustard poultices and hot fomentations unquestionably contributed much to allay the subacute inflammatory action without having recourse to blood-letting or other more severe measures.

II.—*Tic-Douloureux of fifteen years' standing dependent on Disordered Function of the Digestive Organs.*

Mrs M., about 40 years of age, has been a martyr to severe tic-douloureux of the supra-orbitar branch of the trifacial nerve for a period of at least fifteen years. She could give no very distinct account of the mode in which the disease originated, but attributed it to sudden exposure to cold, after having been overheated in dancing. It had been early recognized as a decided and severe case of facial neuralgia, and she had been subjected to many and various modes of treatment by different medical practitioners to whom she had applied, without experiencing anything like decided relief; and, as the disease had been getting more severe, and the agony more insufferable, of late years she despaired altogether of being cured. As her husband feared her mind would give way, under the agony from the pain, and as she was actually quite deranged during the fits, I was asked to visit her on the 7th of May 1849.

I then learned that the fits of neuralgia were always most severe in spring, and that for several years past they became at that time so agonizing and so protracted as quite to unfit her for the duties of life. That day, and for some months previous to my seeing her, the paroxysms of neuralgia had commenced about eight in the morning, by a creeping and thrilling sensation over the left side of the head, concentrating about the *supra-orbitar foramen,* and extending inwards and backwards towards the base of the eye and brain. The pain rapidly increased in intensity, till it became insufferable, as if the head were squeezed in a vice of burning iron, and she got quite deranged. In this state she lay, tossing in restless extasy, till about two o'clock in the afternoon, when the pain began to abate, and by three o'clock had in general left her, weakened, prostrated in mind and body, and unfit for anything. During the summer and autumnal months, the neuralgic fits were in general of shorter duration, rarely exceeding half an hour in continuance, and not so severe, but were more liable to return on excitement, or after meals.

The tongue had a smooth, swollen or œdematous, glassy aspect, with a thin viscid white coating, through which the papillæ projected with a flattened button-like appearance. The breath had a peculiar heavy odour. The skin was soft, clammy, very loose, as if wanting both in elasticity and in tonicity. The complexion was pasty. The bowels reported irregular, but generally costive, often five days passing without a motion, at other times slight diarrhœa for a-day or two. The catamenia were stated to be regular, but watery and rather profuse. The pulse was 60, languid, and very compressible. There was no appetite. She suffered from flatulence after meals, and had faulty digestion.

As the *primæ viæ* appeared in this case to be chiefly at fault, and the disease presented a decided periodic tendency, there seemed reasonable grounds for holding out prospects of relief, to a certain extent at least. She was, therefore, ordered to take two grains of bebeerine at six o'clock evening, a laxative of a particular kind at bed-time, and other two grains of bebeerine at seven o'clock morning daily till relief was experienced. The laxative consisted of jalap fifteen grains, magnesia and carbonate of soda, of each twelve grains, and calomel half a grain.

After two days, viz. on the 9th of May, I repeated my visit at eleven o'clock morning, and found her so much relieved, that, instead of lying in bed insensible, or deranged with the pain, she was dressed, and was sitting up. The pain, though severe, and confined to the branches of the *supra-orbitar nerve* was bearable, and she thought from the sensation that it was beginning to abate for the day. The laxative had operated powerfully, bringing away a large quantity of hardened scybalæ of a disgusting odour. She and her attendants mentioned that, since the first laxative operated, the pain had not been so severe as to deprive her of her senses, and even the first day was diminished in duration about three hours. She was directed to continue the medicines for at least a-week regularly, then only take the laxative every other night, and the pills of bebeerine in the morning only.

By the 20th May, the neuralgic pains had entirely left her, and for several mornings previous had been so trifling as to give no great annoyance. The tongue was now clean, and more healthy looking. The complexion was clear, and had lost almost entirely its pasty look. The appetite was good, the digestive powers had improved, and she suffered little from flatulence. The laxative was therefore diminished by one-half, and directed to be taken every third night, one grain of bebeerine being continued every morning for a week longer. After this the bebeerine was stopped, and a laxative of magnesia occasionally so as to regulate the bowels. By following these directions, and having recourse to her old laxative whenever the pain threatened to return in consequence of exposure to cold, she has escaped all protracted suffering up to this date, 22d February 1850, and finds the disease quite manageable.

III.—*Neuralgia of three years' continuance dependent on disordered action of the Digestive Organs.*

Miss R., an old lady, without teeth, while eating raw oysters with vinegar and pepper, was suddenly seized with acute pain in the gum, over the left lower jaw, resembling severe toothach. The pain was so sudden and violent, that she imagined it must have been occasioned by a piece of oyster shell wounding one of the nerves of the now defenceless gum; and as the slightest touch over a particular spot excited a fresh paroxysm of pain, nothing would convince her that a fragment of shell was not there pricking the nerve. Her friends examined the gum most carefully, but could not perceive that it had received the slightest injury. In the daily expectation that the supposed fragment of shell would work itself out of the gum, she did not apply for medical aid; and three years of varied suffering passed over her head, before the paroxysms of pain became so severe and frequent that they could be no longer borne.

When called to see her on the 3d of February 1849, I found her suffering acute agony from a pain which seized her suddenly and without warning, in the gum of the left lower jaw, at a point corresponding to the position of the last molar tooth. The pain was compared to a red-hot knife piercing the jaw; it continued with great intensity for about three minutes, then wore off, striking forwards to the chin, and backwards to the ear. The paroxysms were always excited by eating, and were most severe and frequent for about two hours after taking food. Taking wine or brandy greatly aggravated the severity and increased the frequency of the paroxysms. After a meal, pressure on the gum, or speaking were almost sure to induce the neuralgia; but after this the gum could in general be freely handled without inducing the pain. The gum was quite healthy in appearance, smooth, neither injected nor swollen more than the healthy side, and firm over the jaw-bone; without the slightest appearance of cicatrix or any prominence to indicate the lodgement of a foreign body within it. The tongue was coated with a thin whitish viscid-looking fur; the breath had a heavy acid odour; and the bowels were habitually very costive, sometimes not moving for five and even eight days. The appetite was bad, the pulse languid and slow, as might be expected in an old person; the sleep troubled, and disturbed with dreams.

This case also appeared to be one where the disease originated in, and was dependent on, disordered function of the digestive organs. It had suddenly attacked her when eating an acid meal of raw oysters; it was aggravated by the articles of diet received into the stomach, and notably so both in severity and frequency by those drinks (wine and brandy) which, in an unhealthy stomach, more speedily run to acidity. She was therefore directed to take daily, at bedtime, a gentle laxative of magnesia, carbonate of soda, and chalk with mercury powder, and morning and afternoon two grains of sulphate of quinine. Within a week, the neuralgic pain had almost completely left her, not seizing her oftener than twice or thrice daily, and then only lasting for a moment. The quinine was therefore stopped, and Henry's calcined magnesia substituted for the laxative; the neuralgic affection from day to day wore away, and for the last nine months she has had no recurrence of it.

These two cases were selected from several others, as clear instances of the close connection between *tic douloureux* and disordered state of the functions of the alimentary canal. I am quite aware that some late writers on this subject have positively asserted that they "have never seen one case of neuralgia referrible to such an origin;" but, in cases of this kind, facts are of much greater importance than opinions; and the above cases appear to demonstrate so clearly that such a connection does exist, that I feel inclined on the other hand to assert, that, in the great majority of cases which occur, the *fons et origo mali* lies with the digestive organs. If a severe and undoubted case of neuralgia, of fifteen years' standing,—one, too, deemed incurable by several practitioners,—be absolutely cured by restoring the functions of the digestive organs,—again attacks, or threatens to attack, whenever the bowels are neglected, and is again and instantly removed by the use of laxatives of a particular kind, independent altogether of antiperiodics,—I leave the profession to draw the conclusion as to the cause and origin of the disease. I have never yet seen a case of *tic douloureux* (not depending on organic disease) in which the digestive organs were not more or less disordered; and since I adopted this view of the disease, and treated it accordingly, I have not yet met with one case which proved rebellious to treatment. Indeed I am not quite sure that the antiperiodics (quinine, bebeerine, and iron) in common use in this disease, when they do aid in removing it, do so more by virtue of their action in improving the tone of the stomach and bowels, than through their antiperiodic property.

Only three days ago (20th February), when looking over my notes of these cases, I was suddenly called to see the most striking instance I have yet witnessed of the close connection between neuralgia and disorder of the digestive organs. A young boy, of about five and a-half years of age, had just finished his dinner, when he suddenly screamed out and threw himself on the ground in the greatest agony. His screams were frightful to hear, and he appeared quite distracted, and on the point of going into convulsions, from pain referred to the back molar tooth of the lower jaw, which tooth appeared perfectly sound. I learned that for some time past, he had been much troubled with indigestion, acidity of the stomach, and irregularity of the bowels, and that many articles of food, such as beef, potatoes, vegetables, or wine, beer, and tea, caused great uneasiness, feverishness, with restlessness, and starting during sleep. He had that day, however, seemed better than usual, and at his urgent request had barley vegetable broth, boiled beef, and a suet dumpling.

Coming fresh from the perusal of the above cases, it appeared to me quite clear, that I had to deal with a case of neuralgia (call it acute toothach if you will), dependent on a disordered state of the digestive organs, more immediately excited by an overloaded stomach, probably accompanied by acidity. A sulphate of zinc emetic was therefore immediately administered, which cleared the stomach of a large quantity of soup of a strong acid odour, and all he had eaten for dinner. As soon as the vomiting was over, he was caused to swallow a dose consisting of chalk, magnesia, carbonate of soda, and cinnamon powder. He had not swallowed it two minutes when the neuralgic pain ceased, and has not since returned.

These cases, then, should have the effect of inducing the profession to direct their attention more to the connection between functional disorder of the digestive organs and neuralgic affections, as I feel persuaded from my own experience that the more the subject is attended to the closer will the connection be found to be. Rheumatic must not, however, be confounded with neuralgic affections, or very false conclusions may be arrived at, as the pathology and treatment of these so far kindred affections are entirely different.

(*To be continued.*)

ART. VIII.—*The Blood-Corpuscle considered in its different Phases of Development in the Animal Series. Memoir* II. —*Invertebrata.* Received 8th May, read 19th June 1845. By T. WHARTON JONES, F. R. S., Lecturer on Anatomy, Physiology, and Pathology at the Charing-Cross Hospital, &c. (Philosophical Transactions of the Royal Society of London for the year 1846, Part I. London, 1846.) [Continued from Volume sixty-seventh, p. 548.]

THE invertebrate animals, of which I am here about to consider the blood-corpuscle in its different phases of development, are some of the more readily procurable examples of the divisions Annulosa and Mollusca. The order in which I purpose to proceed is from the higher to the lower classes of each division, an order the contrary of that adopted in the case of the Vertebrata.

ANNULOSA.

1. My examples of Annulosa are from the classes of Crustacea, Arachnida, Insecta, and Annelida.

Examination of the Blood-corpuscles of Crustacea.

2. As examples of Crustacea I have taken Crabs and Lobsters.

Blood-corpuscles of Crabs.

3. By snipping off the end of one of the legs of a crab, the blood flows freely out from the stump, and is thus readily obtained.* A small quantity of the blood being received on a plate

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* The blood as it flows from the animal appears of a pale reddish gray or neutral tint; when collected in a watch-glass, it separates into a spongy-looking mass

of glass direct from the animal, and forthwith examined under the microscope, it is seen to contain a very considerable number of corpuscles.

4. *Kinds of Corpuscles.*—Both granule-cells and nucleated cells—the latter the more numerous—may be distinctly recognised.

5. *Shape of the Cells.*—If very great expedition has been employed in receiving the blood as it flows from the animal on the plate of glass, spreading it out and transferring it to the microscope for examination, the granule-cells may be seen to be of an elongated oval shape, the nucleated cells spindle-shaped (figs. 1 and 6.) These shapes, however, are speedily changed.

6. As regards the granule-cell, it tends to become circular (figs. 2 and 3), but it is also soon observed that its cell-wall bulges out here and there into round processes which again subside, whilst another part of the cell-wall bulges out in the same way. This change of shape, it will be perceived, is similar to that which I particularly described in the case of the granule-cell of the blood of the skate, only it is to be remarked, that it is not accompanied by such a well-marked movement of the contained granules. Besides bulging out into round processes, the cell well may be seen to shoot out into cilia-like processes also (fig. 4.)

7. As regards the nucleated cell, it also first tends to become circular, and then shoots out its wall into processes which are usually more cilia-like than in the case of the granule-cell; and being in all directions like radii, the cell comes to present a stellate appearance (figs. 7, 8 9.) Sometimes a cell is seen to shoot out into processes in two principal directions only, these processes again shooting out into smaller, so that the cell acquires a caudate form. Between this shape and the former there is every intermediate degree.

8. *Size of the Cells*—When first examined, and before their shape has become changed, the granule-cells are about $\frac{1}{1800}$ of an inch long by about $\frac{1}{3600}$ of an inch broad. The nucleated cell is rather less in size, or perhaps with about the same length; it is not quite so broad.

9. *Structure of the Granule-cell.*—There are both coarsely and finely granular stages of the granule-cell. The granules appear more or less fused together, and are of a slightly greenish yellow colour when the microscopical examination is made by day-light.

10. By the addition of water the cell-wall is brought out very

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and a serous fluid. This spongy-looking mass, which is of a slight pink colour, consists principally of the corpuscles aggregated, there being little spontaneously coagulable material in the plasma to form a true clot. The serous fluid, which is bluish by reflected light, and reddish by transmitted light, is coagulated by heat, and also by *acetic acid.*

distinctly. Imbibing the water, the cell becomes distended and acquires a circular shape, and this as well after it has shot out into processes as when the water is added to the blood just drawn, and before the cell has lost its original shape. Fig. *5 a.* represents a granule-cell as acted on by water.

11. In consequence of acetic acid producing a copious white curdy precipitate on its addition to the blood, some difficulty was at first experienced in studying the action of that acid on the blood-cells, but by previously diluting the blood with much water, the precipitate was not considerable enough to prevent the observation of the action of the acetic acid on the blood-cells.

12. When the acid comes into contact with the granule-cell, the granulous mass is dissolved, the granules disappearing one after the other, as already described in regard to the granule-cell of the skate, &c. After the solution of the granulous mass, there is discovered in the interior of the cell, a cellæform nucleus about $\frac{1}{7000}$ of an inch in diameter (fig. *5 b.*)

13. A clear spot which may be observed on the side of the granule-cell in blood just drawn (figs. 1, 2, 3), and which is similar to that presented by the granule-cell of the blood of the skate, appears to be, as in the skate, produced by the nucleus peering through a pore in the granulous mass.

14. *Structure of the Nucleated Cell.*—By the action of water, the nucleated cell is distended and rendered circular even after it has shot out processes and acquired a stellate appearance. During this action of the water, the processes of the cell-wall may be observed to become broader and shorter, and to run into each other until the outline of the whole cell is circular, as in fig. 10. Within the cell thus distended, the cellæform nucleus is seen circular, and about $\frac{1}{3500}$th of an inch in diameter.

15. By the addition of dilute acetic acid, the nucleated cell is not further materially affected.

16. Although a decided coloured stage of nucleated cell does not exist in the blood of the Crab, it is proper to observe that many of the nucleated cells transmit the light through their interior of a very slight red tint. The circumferential doubling of the cell-wall transmiting the light quite colourless, forms with the interior a well-marked contrast. Though the red tint is more marked by lamp-light, it is sufficiently distinct by day-light. It has been above mentioned, that when the corpuscles collected in a mass are viewed by reflected light, they also appear slightly pink.

17. Besides granule-cells and nucleated cells such as have now been described, cells occur in which the cellæform nucleus is already visible, but still surrounded by some granulous matter (fig.

11.) These may be viewed as cells in a state of transition from granule to nucleated cell.

18. It may thus be concluded, that in crabs the blood-corpuscle presents itself in two different phases of development, as in the oviparous Vertebrata, viz. the phases of granule-cell and of nucleated cell, the granule-cell being the first phase of development, the nucleated cell the second phase.

19. Floating about amongst the blood-corpuscles, there are seen a few elementary granules, the larger of which, from $\frac{1}{8000}$th to $\frac{1}{12000}$ of an inch in diameter, have the form of biconcave circular discs.

Blood-corpuscles of the Lobster.

20. The description which has now been given of the blood-corpuscles of the crab is in all respects applicable to those of the lobster.*

Examination of the Blood-corpuscles of Arachnida.

21. It was the *common spider* the blood of which I examined; and the mode in which I obtained the blood was the same as that already stated in regard to the crab and lobster, viz. by snipping off one of the legs and receiving on a plate of glass the minute drop of blood which oozed out.

22. The blood-corpuscles of the spider are almost identical with those of the crab and lobster. There are both granule-cells and nucleated cells,—the latter the more numerous,—which, when examined immediately on the blood being drawn from the animal, present, the former an oval, the latter an elliptical shape (figs. 1, 2, and 7.) These shapes, however, are in general soon lost. The cells become roundish, their cell-wall at the same time shooting out into processes. This shooting-out of the cell-wall into processes is more especially presented by the granule-cells (fig. 3), for in the case of the nucleated cell, it might be said that the cell-wall in general rather becomes shrivelled and collapsed than shoots into cilia-like processes (fig. 8.)

23. The size of the corpuscles is much the same as in the crab.

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* The changes of form which the blood-corpuscles of the lobster, like those of the crab, undergo after the blood is drawn, were described and delineated by Hewson as accurately as his microscope appears to have enabled him to observe them. Hewson also mentions what I have found to be the case, that the blood of the lobster, "after being some time exposed to the air, jellies, but less firmly than the blood of more perfect animals." In this respect the blood of the lobster differs from the blood of the crabs which I examined; the blood of the latter, as I have above stated, not containing a sufficient quantity of spontaneously coagulable material to form a perfect clot. It may be proper to remark that it was in the beginning of winter when I examined the crab's blood, and in the beginning of spring when I examined that of the lobster.

24. There are both coarsely and finely granular stages of the granule-cell (figs. 1 and 2), and also cells in transition from the granule to the nucleated phase.

25. By the action of water, the cell-wall of both granule and nucleated cell is distended and brought out in the same way as in the case of the blood-cells of the crab (figs. 4, 5, and 9), and by the action of acetic acid the granules of the granule-cells are broken up and dissolved, leaving the nucleus exposed (fig. 6.)

26. In regard to a coloured stage of nucleated cell, what was above said in the case of the crab (par. 16), is applicable here.

27. As in the blood of the crab also a few elementary granules are seen floating about.

Examination of the Blood-corpuscles of Insecta.

28. In entering on the examination of the blood-corpuscles of insects, the advantage of having first examined those of crabs and spiders is strongly felt; for the blood of those animals being readily and certainly obtainable free from admixture with foreign particles, which might be confounded with blood-corpuscles, the characters of their blood-corpuscles will serve as a guide by which to recognise their analogues in the blood of insects and other invertebrate animals, the conformation and structure of the bodies of which are such that we cannot be sure that the blood obtained from them is quite free from foreign admixture.

29. As examples of perfect insects I have taken beetles, and as examples of chrysalises I have taken those of the cabbage butterfly.*

Blood-corpuscles of Beetles.

30. The matter examined as the blood of these insects was the clear greenish yellow fluid which exuded when a small wound was made in the body of the animal.

31. In this fluid there were recognised, amidst a number of other corpuscles, the nature of which will be inquired into below, corpuscles corresponding in essential characters to the granule and nucleated blood cells of the crab and spider.

32. *Granule Blood-cells.*—These were, when seen immediately on the blood being drawn, of an elliptical or oval shape (fig. 1,) but they soon became round. In this state some might be seen with the cell-wall shot out into small cilia-like processes (fig. 2.) By the action of water, the cell becoming distended, these processes disappeared (fig. 3.)

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* I have also examined the blood-corpuscles of the caterpillar, and found them in essential particulars the same. Having, however, as yet obtained but one specimen of a caterpillar of the cabbage butterfly, I am not able to give here a description and delineation of the blood-corpuscles. This, however, I shall be able to do on another occasion.

33. The size of the cells under consideration was on an average $\frac{1}{2000}$th of an inch long by $\frac{1}{4000}$th broad.

34. The cells were most of them pale-looking, but some presented coarser and more refracting granules than others.

35. *Nucleated Blood-cells.*—These were much more numerous than the granule blood-cells. When the blood was first examined on being drawn, the cellæform nucleus, of an oval shape, measuring $\frac{1}{3650}$th by about $\frac{1}{7000}$th of an inch, and reddish in its interior, was the part most distinctly seen. The cell-wall, which might sometimes be seen of an elliptical shape (fig. 4,) was in general already found shrivelled and collapsed around the cellæform nucleus, or shot out into processes (fig. 5.) It was extremely pale and not always very readily distinguished. On the addition of water, however, it became distended and was then pretty distinctly seen (fig. 6.) In this state it was circular and measured about $\frac{1}{2000}$th of an inch in diameter.

36. In regard to the other kinds of corpuscles contained in the blood of the beetle, they were, 1st,—a few corpuscles resembling the cellæform nuclei of the nucleated cells, but around which no cell-wall could be detected; 2d, some oil-globules; 3d, a great number of elementary granules in size from about $\frac{1}{7000}$th of an inch in diameter downwards, the larger being biconcave, circular, and of a yellowish or reddish colour.

Blood-corpuscles of the Chrysalis of the Cabbage Butterfly.

37. The matter examined as blood was the clear green fluid which flowed out on slight pressure, when a small point of the anterior end of the chrysalis was snipped off.

38. In this fluid there were both granule-cells and nucleated cells (figs. 7, 8, 9, 10, 11, 12,) together with elementary granules.

Examination of the Blood-corpuscles of Annelida.

39. As examples of Annelida, I have taken the common earthworm and the medicinal leech.

Blood-corpuscles of the Earth-worm.

40. The blood was most readily obtained for examination from the abdominal vessel, but in abstracting it, care was required to guard against its becoming mixed with the secretion poured out from the skin in great abundance when the animal was wounded.

41. The corpuscles of the blood of the earth-worm are remarkable for their great size, being on an average $\frac{1}{1100}$th or $\frac{1}{1200}$th of an inch in diameter. There are both granule and nucleated cells.

42. *Granule Blood-cells.*—Of these there are both coarsely

and finely granular stages. I have not observed the cell-wall of the granule-cell in the coarsely granular stage to shoot out into more than perhaps a single bud-like process (fig. 1); but in the finely granular stage, the cell-wall shoots out into so many cilia-like processes, that the cell soon after the blood is drawn presents a stellate form (fig. 2), the granulous mass about $\frac{1}{1800}$th of an inch in diameter occupying the centre.

43. By the action of water the granule-cells become uniformly distended (figs. 3 and 4), the stellate form into which the cell in the finely granular stage had fallen disappearing. In this state of distension the cells measure $\frac{1}{1100}$th or $\frac{1}{1200}$ of an inch in diameter, but their granulous contents do not fill their whole interior.

44. Acetic acid causes the granules of the cell in this coarsely granular stage to break up, but does not dissolve their substance. The granulous contents of the cell in the finely granular stage it renders more transparent.

45. *Nucleated Blood-cells.*—Instead of perfect nucleated cells, the cells about to be described under this head might, perhaps, rather be said to be cells in transition from the granule-cell phase, inasmuch as the nucleus, though quite evident, is still surrounded by granular matter.

46. The cells under consideration have always been collapsed when first seen, though the blood was examined as quickly as possible after being drawn. But I have observed them in the act of shooting out their cell-wall into processes (fig. 5), like the granule-cell in the finely granular stage.

47. By the action of water, the processes are made to disappear by the cell becoming uniformly distended. In this state the cell measures about $\frac{1}{1100}$th of an inch in diameter, and in its interior is seen the cellæform nucleus about $\frac{1}{3600}$th of an inch in diameter, with a finely granulous mass surrounding it (fig. 6). This granulous mass is not dissolved entirely by acetic acid, but it is rendered more transparent.

48. The red colour of the blood of the earth-worm is, as is known, seated in the plasma; but it is to be remarked that some of the nucleated cells appear very slightly tinged, as also the nuclei, in their interior.

49. Besides the nucleated cells now described, corpuscles are met with altogether like their nucleus and its surrounding granulous mass, both in form and size.

50. Lastly, there are a few corpuscles like perfectly free nuclei, and a considerable number of elementary granules, the larger, of the form of biconcave circular discs.

Blood-corpuscles of the Medicinal Leech.

51. The blood examined was obtained by making incisions on the back or side of the animal after having carefully wiped the surface of the skin. In making these incisions care was taken not to cut into the stomach, or its cæcal appendages, in order to obviate the possibility of foreign blood which might have been taken in as food, becoming mixed with the real blood of the animal itself.

52. In the first place, it is worthy of remark, that whilst the corpuscles of the blood of the earth-worm are the largest which I have yet found in any invertebrate animal, the corpuscles of the blood of the leech are the smallest.

53. When the blood of the leech is examined under the microscope as soon after its abstraction from the animal as possible, numerous corpuscles are seen, having the appearance of very pale, shining, colourless, fusiform filaments, about $\frac{1}{2000}$th of an inch in length, and about $\frac{1}{7000}$th of an inch in breadth, less or more (fig. 9), suspended in the red-coloured plasma. Very soon, however, the corpuscles are seen gradually to become shorter and somewhat broader, until at last they acquire an irregular circular form (fig. 10).

54. In this state the majority of the corpuscles appear to be composed of a nucleus surrounded by a collapsed and shrivelled cell-wall. And that this is so is reduced to a certainty by the addition of water, which causes the cell to become distended and to acquire a circular form, whilst the outline of the cellæform nucleus appears more distinctly defined (figs. 11 and 12).

55. Thus distended, the cell is from $\frac{1}{3000}$th to $\frac{1}{3600}$th of an inch in diameter, and the nucleus $\frac{1}{7000}$th, or more.

56. Many of the cells transmit the light slightly tinged red, as if there was some colouring matter lining their interior (fig. 12).

57. The corpuscles of the blood of the leech now described, it will have been observed, are *nucleated cells.*

58. *Granule-cells.*—Corpuscles in the blood of the leech referrible to this head are few in number. Examples of them are delineated in figs. 7 and 8.*

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* I subjoin here a description of the corpuscles of the blood vomited by leeches which have never been used.

This blood I found to be composed of a coloured plasma, and numerous corpuscles having a considerable resemblance to those of the blood of the animal itself after having lost their original fusiform shape, and become somewhat distended by water. On the whole, however, the corpuscles of the blood in question were somewhat larger, and appeared perhaps better defined, than those of the leech itself, especially the granule-cells, which were also more numerous, though still not so numerous as the nucleated cells.

Of the granule-cell there were both coarsely and finely granular stages (figs. 13 and 14). There were cells in transition from the granule to the nucleated phase (fig. 15). The nucleated cells (figs. 16, 17, and 18) were most of them circular, but some were oval. Most appeared tinged red in their interior; those which did not had their cell-wall so very pale that it was apt to be overlooked.

MOLLUSCA.

59. My examples of Mollusca are confined to the classes of Gasteropoda and Acephala,—from the former I have taken the Whelk (*Buccinum magnum*),—from the latter the Mussel (*Mytilus edulis*), in which to examine the blood-corpuscles.

Blood-corpuscles of the Whelk.

60. The shell having been broken to pieces from around the animal, the blood was readily obtained from the great vessels of the heart, or from the heart itself; but, notwithstanding every care in abstracting it, the blood was not always quite free from admixture with foreign particles, such as ciliated epithelium-cells and the like.

61. *Kinds of Corpuscles.*—There were granule-cells and nucleated cells essentially similar to those of the blood of Annulosa.

62. *Granule-cells.*—By the time the blood could be examined, the granule-cells had, for the most part, become agglomerated together in groups, and their cell-wall was seen already shot out into processes (fig. 1).

63. Both coarsely and finely granular stages of the granule-cell were to be recognised.

64. The size of the granule-cell was on an average from $\frac{1}{3000}$th to $\frac{1}{2000}$th of an inch.

65. By the action of water the cell became uniformly distended (fig. 2) to the size of about as much as $\frac{1}{1500}$th of an inch in diameter and then burst, the cell-wall disappearing and leaving the granulous contents in a mass with the nucleus visible in the centre (fig. 3).

66. *Nucleated cells.*—Some of these were seen with the cell-wall shot out into processes (fig. 4); some not (figs. 5, 6, and 7).

67. Those cells of which the cell-wall did not shoot out into processes, were for the most part circular (figs. 5 and 6); but some also occurred which appeared to be elliptical (fig. 7), though these might have been circular cells seen somewhat raised up on edge. The cells under consideration often presented a finely granulous matter in their interior, and some were slightly tinged of a red colour. Their size was about $\frac{1}{2500}$th of an inch in diameter.

68. Those cells of which the cell-wall shot out into processes, when uniformly distended by water, were larger, measuring about $\frac{1}{1650}$th of an inch in diameter (fig. 4 *a*).

69. The nucleus, oval or circular, and reddish in the interior,

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The blood contained in the stomach of leeches which have not been used, and of which I have now briefly described the corpuscles, from what animal is it derived? The resemblance of the blood to that of the animal itself suggests the probability that medicinal leeches suck the blood of some other kind of leech. That they suck each other's blood has been positively denied by Dr RAWLINS JOHNSON.

measured on an average from $\frac{1}{4800}$th to $\frac{1}{3650}$th of an inch, these being the means when the nucleus was oval.

70. Besides the corpuscles now described, there were a few like free nuclei, and a great quantity of elementary granules of different sizes, the larger biconcave and circular.

Blood-corpuscles of the Mussel.

71. The way in which I obtained the blood of the mussel was this:—I first removed one valve of the shell with as little injury to the animal as possible, and allowed the contained fluid to drain away; I then opened the great vessel proceeding from the heart to the anterior part of the body, and introducing the microscopical forceps towards the heart, took up enough of the blood for one examination. Notwithstanding this careful procedure, the blood was often mixed with foreign corpuscles, such as the spermatozoa and ova of the animal.

72. *Kinds of Blood-corpuscles.*—There were in the first place *granule-cells*, which, like those of the blood of the whelk, were found by the time the blood could be examined, already for the most part agglomerated together, and presenting their cell-wall shot out into processes (figs. 1 and 2). The granulous contents appeared as if fused, forming a clear, more or less strongly refracting mass.

73. By the action of water this mass was broken up into separate granules, and at the same time the cell was observed to become gradually distended and rendered uniformly circular by the undoing of the processes into which its wall had shot. This undoing of the processes I have often, as in the case of the blood-cells of other animals, watched step by step.

74. When the cell had become fully distended, the granulous contents were usually next seen to escape by the bursting of the cell-wall, and to accumulate around its outside, whilst a nucleus from about $\frac{1}{7000}$th to $\frac{1}{4000}$th of an inch in diameter, and now for the first time seen, remained in the interior; the cell-wall, different from that of the granule-cell of the whelk, continuing quite visible and otherwise entire (fig. 3).

75. Such was the condition in which the granule-cells presented themselves in the blood of fresh mussels. In the blood of mussels which had been kept for some days in the house without food or change of water, but still alive though weak, the granule-cells were found, for the most part, to continue uniformly distended after the blood was drawn, their cell-wall not shooting out into processes (figs. 4 and 5). The contents appeared at the same time more broken up into granules. In this state the size of the cells was from $\frac{1}{1800}$th to $\frac{1}{2000}$th of an inch in diameter. They appeared in both coarsely and finely granular stages.

76. Cells in which the nucleus is already distinctly visible without the application of any reagent, were few in number in the blood of perfectly fresh mussels, but in the blood of mussels which had been kept some days, they presented themselves in considerable numbers (figs. 9, 10, 11, and 12).

77. The corpuscles in which a nucleus had thus become distinctly visible were, as they occurred in the blood of fresh mussels, distinguished by the following characters:—Circular but sometimes ovoid, somewhat prominent in the middle, with a uniformly and strongly marked contour, of an opaline aspect, and measuring on an average about $\frac{1}{2400}$th of an inch in diameter, many being both smaller and larger (figs. 6 and 7).

78. By the action of water the contents of these corpuscles, previously homogeneous looking, adherent to the cell-wall, and concealing the nucleus, became broken up into granules, so that the cell-wall and a nucleus came more or less distinctly into view, with characters similar to those presented by them after the animals had been kept some time as above mentioned, and which I now proceed to describe (fig. 8).

79. The cell was generally circular but sometimes oval, and its wall did not shoot out into processes. It measured on an average $\frac{1}{2400}$ of an inch, some more, some less.

80. The nucleus was circular or oval. In the former case it measured about $\frac{1}{3600}$ of an inch in diameter; in the latter, the mean of the two diameters was on an average the same as this. Some were much elongated but narrow, others short but broad.

81. The circular cells had the nucleus either circular or oval, but when the cell was oval the nucleus was oval also, and usually elongated and narrow. Sometimes the oval nucleus appeared almost free, the cell-wall very much contracted, being attached to one side only.

82. In addition to the corpuscles now described, there were observed corpuscles very pale and few in number, apparently the nuclei of the nucleated cells become free and in process of disappearing.

83. Lastly, there was a great number of elementary granules: the larger, of the form of biconcave circular discs, measuring on an average $\frac{1}{5000}$th or $\frac{1}{7000}$th of an inch in diameter, less or more.

84. It thus results that in all the invertebrate animals in which the blood has now been examined, the blood-corpuscle presents itself in the phases of granule-cell and of nucleated cell; that in the phase of granule-cell it occurs in more or less well-marked coarsely and finely granular stages; that in the phase of nucleated cell it occurs in the uncoloured stage; but that, as regards a

coloured stage, though such cannot be said to be altogether wanting in some of the animals, the coloration of the cell is in a very slight degree. The cellæform nucleus is found more frequently and more decidedly coloured.

85. It also results that corpuscles exist in transition from the phase of granule-cell to that of nucleated cell.

86. It farther results that corpuscles occur in the blood of some of the animals, which appear to be the nuclei of the nucleated cells become free. Such, however, are to be viewed as abortions rather than as examples of a third phase, or phase of free cellæform nucleus.

87. Lastly, it results that corpuscles are met with in greater or less numbers belonging to the category of the lowest forms of organic elements, viz. elementary granules.

EXPLANATION OF THE PLATE.

ANNULOSA.

CRUSTACEA.—CRABS.

First Phase.—Granule-Cell.

Fig. 1. Granule-cell as it appears when the blood is examined immediately on being drawn.

Figs. 2 and 3. Granule-cells changed in shape as they appear very soon after the blood has been been drawn.

Fig. 4. Granule-cell, with the cell-wall in view at the circumference, and shooting out bud-like processes here and there.

Fig. 5 *a*. Granule-cell after being acted on by water.

Fig. 5 *b*. Granule-cell after being acted on by acetic acid. The granules have all disappeared by solution, and a small cellæform nucleus has thus come into view.

Second Phase.—Nucleated Cell.

Fig. 6. Nucleated cell as it appears when the blood is examined immediately on being drawn.

Figs. 7, 8, 9. The nucleated cell in different degrees of change of shape.

Fig. 10. Nucleated cell after being acted on by water.

Fig. 11. Another, after being treated in the same way, but still presenting some remains of the granules of the first phase, and a smaller cellæform nucleus than the preceding.

ARACHNIDA.—SPIDER.

First Phase.

Fig. 1. Granule-cell, coarsely granular stage.

Fig. 2. Granule-cell, finely granular stage; both as seen when the blood was examined immediately on being drawn.

Fig. 3. Granule-cell changed in shape, and with the cell-wall shot out into processes.

Figs. 4 and 5. Granule-cells in coarsely and finely granular stages distended by water.

Fig. 6. Granule-cell after the granules have been dissolved by acetic acid.

Second Phase.

Fig. 7. Nucleated cell as it appeared when the blood was examined immediately on being drawn.

Fig. 8. The same, changed in shape, as it appeared soon after the blood was drawn.

Fig. 9. The same, distended by water.

INSECTA.—BEETLE.

First Phase.

Fig. 1. Granule-cell as it appeared when the blood was examined as soon as possible after being drawn.

Fig. 2. The same, changed in shape—the cell-wall shot out into processes—as it appeared soon after the blood was drawn.

Fig. 3. The same, after the action of water.

Second Phase.

Fig. 4. Nucleated cell, as it may sometimes be seen when the blood is examined immediately on being drawn.

Fig. 5. The same cell, changed in shape. The cell-wall extremely pale, shrivelled, and shot out here and there into processes.

Fig. 6. The same, distended by water.

INSECTA.—CHRYSALIS OF CABBAGE-BUTTERFLY.

First Phase.

Figs. 7 and 8. Granule-cells in coarsely and finely granular stages.

Fig. 9. Granule-cell, with the cell-wall shot out into processes.

Second Phase.

Fig. 10. Nucleated cell of its original shape.

Fig. 11. The same, with the cell-wall shrivelled and shot out into processes.

Fig. 12. The same, distended by water.

ANNELIDA.—EARTH-WORM.

First Phase.

Fig. 1. Granule-cell, coarsely granular stage. The cell-wall shot out into a single bud-like process.

Fig. 2. Granule-cell, finely granular stage. The cell-wall shot out into cilia-like processes.

Figs. 3 and 4. The same cells, distended by water.

Second Phase.

Fig. 5. Nucleated cell, with its cell-wall shot out into processes.

Fig. 6. The same, distended by water.

ANNELIDA.—MEDICINAL LEECH.

First Phase.

Figs. 7 and 8. Granule-cells.

Second Phase.

Fig. 9. Nucleated cell, as seen when the blood is examined immediately on being drawn.

Fig. 10. The same, changed in shape, as it appeared soon after the blood is drawn.

Figs. 11 and 12. Nucleated cells distended by water.

Blood vomited by Medicinal Leech.

Figs. 13 and 14. Granule-cells, coarsely and finely granular stages.

Fig. 15. A transition cell.

Figs. 16, 17, and 18. Nucleated cells.

MOLLUSCA.

GASTEROPODA.—WHELK.

First Phase.

Fig. 1. Granule-cell, with the cell-wall shot out into processes.

Fig. 2. The same, distended by water.

Fig. 3. The contents and nucleus of the same, set free by the bursting of the cell-wall.

Second Phase.

Fig. 4. Nucleated cell, with the cell-wall shot out into processes.

Fig. 4 *a*. The same, distended by water.

Figs. 5, 6, and 7. Nucleated cells, the cell-wall of which did not shoot out into processes.

ACEPHALA.—MUSSEL.

First Phase.

Figs. 1 and 2. Granule-cells, coarsely and finely granular stages, with the cell-wall shot out into processes.

Fig. 3. Granule-cell after the action of water.

Figs. 4 and 5. Granule-cells from a mussel which had been kept some time in the house.

Second Phase.

Figs. 6 and 7. Cells from the blood of fresh mussels, by the action of water on which a nucleus has been rendered visible, as in fig. 8.

Figs. 9, 10, 11, 12. Nucleated cells found in considerable number in the blood of mussels kept some time, but few in number in fresh mussels.

MEMOIR III.—*Comparison between the Blood-Corpuscle of the Vertebrata and that of the Invertebrata.*

[Conclusion.]

1. In instituting a comparison between the blood-corpuscle, in its different phases of development, of the vertebrata and that of the invertebrata, it is obvious that the examples first taken for the purpose ought to be selected from the lowest class of the oviparous vertebrata on the one hand, and from the highest class of the highest division of the invertebrata on the other.

2. In accordance with this, I proceed to compare together the blood-corpuscles of the skate and crab.

Comparison between the Blood-corpuscle of the Skate and that of the Crab.

3. From the observations above related, it results that the blood-corpuscle of the crab resembles that of the skate in presenting two different phases of development, viz. the phase of granule-cell and the phase of nucleated cell, and that in these two phases respectively the essential points of structure are the same.

4. The blood-corpuscle of the two animals, however, differs in the degree of development which it attains in the phase of nucleated cell. In the crab its development is arrested at the uncoloured stage, or, at the most, at the commencement of the coloured stage. But this absence of nucleated cells in the decided coloured stage in the blood of the crab, it has been seen, is a peculiarity which there is reason to believe is presented by one at least from among oviparous vertebrate animals, viz. *Branchiostoma lubricum.*

5. The blood of this fish may therefore be admitted as probably differing but little in the character of its corpuscles from the blood of the crab, and as constituting in this respect a transition from the vertebrata to the invertebrata.

6. Having thus compared the blood-corpuscle of an animal from the lowest class of oviparous vertebrata with that of one from the highest class of the highest division of the invertebrata, we are prepared to institute a comparison between the blood-corpuscle of the oviparous vertebrata generally, and that of the invertebrata from crabs to mussels.

Comparison between the Blood-corpuscle of the Oviparous Vertebrata generally, and that of Invertebrata.

7. In the oviparous vertebrata, from the skate upwards, it has been seen that the blood-corpuscle in its different phases of development is essentially similar to that of the skate. In the invertebrata, from the crab down as far as we have gone, it has also been seen that the blood-corpuscle in its different phases of development is essentially similar to that of the crab. The only difference, therefore, in essential respects, between the blood-corpuscle of the oviparous vertebrata generally and that of the invertebrata, is the same as that between the blood-corpuscle of the skate and crab, viz., that in the phase of nucleated cell, the latter does not attain to a decidedly coloured stage.

8. Hitherto I have altogether excluded the blood-corpuscle of the mammifera from comparison with that of the invertebrata; but such comparison I now proceed to make.

Comparison between the Blood-corpuscle of Mammifera and that of Invertebrata.

9. In the phase of granule-cell, the blood-corpuscle of the mammifera and that of invertebrata resemble each other in essential points of structure. In the phase of nucleated cell they also agree in the absence of a decided coloured stage; in this respect differing from the blood-corpuscle of the oviparous vertebrata generally and of the early mammiferous embryo.

10. But here the resemblance between the blood-corpuscle of the mammifera and that of the invertebrata ceases. In common with the blood-corpuscle of the oviparous vertebrata, that of the invertebrata differs from the blood-corpuscle of the mammifera in not attaining to a decided third phase. The free cellæform nuclei which appear to exist in the blood of some of the invertebrata, it has been above stated, can only be considered as abortions of such a phase.*

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* From neglecting the precautions above observed in instituting a comparison between the blood-corpuscles of the vertebrata, and those of the invertebrata, Mr Newport, in a communication recently made to the Royal Society, appears to me to have fallen into a very serious error, as to what corpuscles in the blood of insects and other invertebrata are analogous to the "red blood-corpuscles" of the vertebrata.

Overlooking altogether even the well-known difference between the "red blood-corpuscles" of the mammifera and those of the oviparous vertebrata, he assumes the "red blood-corpuscle" of the mammifera as a standard by which to recognise in the blood of insects and other invertebrata, any corpuscles which may be the analogues of the "red blood-corpuscles" of the vertebrata generally.

Accordingly, finding in the blood of insects the elementary granules described in Memoir II. of this series of papers, pars. 19, 27, 36, 38, &c., and not being acquainted with the true nature of such particles, he has been misled by their similarity to his assumed type of "red blood-corpuscles," especially in respect of out-

11. It was at one time supposed that the red colour of the red blood-corpuscles of the vertebrata is dependent on the iron which they contain, but latterly chemists have come to the conclusion, that though iron is essential to the "red corpuscles," it is not so to their colouring matter.

12. In reference to this question, it occurred to me that it would be interesting to ascertain whether the corpuscles of the blood of an invertebrate animal, notwithstanding their slight degree or total want of colour, contain iron.

13. Accordingly I collected some of the corpuscles of the blood of the crab, which it is easy to do in consequence of the mode in which they collect together after the blood is drawn, as described in Memoir II. of this series, and in consequence of the plasma not coagulating. These I dried carefully and sent to my friend, Mr Thomas Graham, Professor of Chemistry in University College, who was so kind as to undertake to test them for iron.

14. According to the examination of this gentleman, it appears that the blood-corpuscles of the crab, though very faintly coloured, contain, nevertheless, a sensible proportion of iron.

15. Of course the presence of iron in the corpuscles of the blood of the crab is not alone a proof that the red colour of the red corpuscles of the blood of the vertebrata is not owing to iron; it is merely a proof that iron in equal quantity may exist in corpuscles without imparting to them a decided red colour, such as is presented by the red corpuscles of the blood of the vertebrata.*

Comparison of the Corpuscles of the Blood of the Invertebrata with the Corpuscles of the Lymph of the Vertebrata.

16. In the very respect in which the blood of the Invertebrata differs from that of the oviparous vertebrata generally, viz. the absence of nucleated cells in the decidedly coloured stage, it resembles their lymph.† (Memoir I., pars. 101 to 105.)

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ward form, which is that of biconcave circular discs, into the conclusion that they are the analogues of the "red blood-corpuscles" of the vertebrata.

The corpuscles in the blood of insects and other invertebrata which, though little or not at all coloured, I have shown to be the true analogues of the "red blood-corpuscles" of the *oviparous* vertebrata, viz. the nucleated cells; Mr Newport considers to be an early stage of the corpuscles which I have described in the same blood under the name of granule-cells, the former, according to him, becoming the latter by the development and accumulation of granules in their interior.

When thus filled with granules, the cells, he appears to believe, burst, and the granules, thus set free, become the alleged analogues of the "red corpuscles" of the vertebrata.

* The coloured plasma of the blood of the crab gave unquestionable indications of iron.

† This conclusion, it is to be particularly observed, is to be distinguished from a view first promulgated by Professor Rudolph Wagner, viz., that the corpuscles

To complete the history of the blood-corpuscle, it remains to inquire into the first formation of the granule blood-cell on the one hand, and into the ultimate fate of the nucleated blood-cell of the invertebrata, and of the oviparous vertebrata, and of the free cellæform nucleus of man and the mammifera on the other; and also to collate the views given of the development of the blood-corpuscle with the theory of cell development in general.

In executing this task I shall have an opportunity of considering several questions which have suggested themselves in the present series of papers, but which it would have been premature to have entered upon.*

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of the circulating fluid of the invertebrata are not any of them analogous to the red blood-corpuscles of the vertebrata, but are all of them analogous to the corpuscles of the lymph, or chyle merely, and that, therefore, the circulating fluid of the invertebrata is "mere chyle, not proper blood," proper blood being, as he defines it, "a red coloured fluid containing characteristic corpuscles."

This view appears to me to be distinction without essential difference, and to have arisen,—1st, from an imperfect acquaintance with the corpuscles of the lymph of the vertebrata, and their relationship to those of the blood of the same division of animals on the one hand, and with the corpuscles of the circulating fluid of the invertebrata on the other; 2dly, from laying too much weight on the presence or absence of colour as a distinctive character; thus making no distinction between the "red corpuscle" of the blood of the mammifera, and the "red corpuscle" of the blood of the oviparous vertebrata, in consequence of their similarity in colour, though there is between them an essential organic difference; whilst, between the "red corpuscle" of the blood of the oviparous vertebrata and the nucleated cell of the circulating fluid of the invertebrata, in consequence of a supposed total absence of colour in the latter, a decided distinction is made, though there is, in fact, between them no essential organic difference.

Waiving, however, the organic difference between the "red corpuscle" of the mammifera and the "red corpuscle" of the oviparous vertebrata, and the organic resemblance between the "red corpuscle" of the oviparous vertebrata, and the little or not at all coloured nucleated cell of the circulating fluid of the invertebrata, I would observe in regard to the presence or absence of colour, that nothing bearing on the question can be inferred from it, even in a physiological point of view, seeing that the nature and use of the red colouring matter of the "red blood-corpuscles" are as yet too little known.

The colour of "red blood-corpuscles" is now acknowledged not to be owing to the iron entering into their composition, and therefore it is not, as has been supposed, a manifestation of the existence of any endowment which may be possessed by the "red corpuscles," by virtue of the iron which they contain. But even if this had been so, the fact above mentioned, of the presence of iron in the uncoloured or but little coloured corpuscles of the blood of the crab, renders it extremely probable that what endowment soever may be possessed by the "red corpuscles" of the vertebrata by virtue of the iron which they contain, may be equally possessed by the corpuscles of the blood of the crab.

Professor Wagner adduces arguments of another kind in corroboration of his view, that the circulating fluid of the invertebrata is "mere chyle, not proper blood;" but which it is scarcely necessary to notice after what I have just said, especially as their force, so far as it is evident, appears to bear more against than in favour of his view.

[* Of the first memoir, by Mr Wharton Jones, on the blood-corpuscle, an account was given in the sixty-seventh volume of this Journal; and it was at the conclusion of that article stated that it was our intention to give a similar account of the second and third memoirs in the ensuing volume. This various circumstances have concurred to prevent from being done. Meanwhile, it appears, that to give the memoirs entire will be quite as just to the author and not less useful to readers, than any abstract, however carefully done.]—Ed.

ART. IX.—*Account of an Instance of Occlusion and Obstruction of the Cavity of the* VENA PORTÆ. *With Remarks on its probable Causes and its Effects.* By DAVID CRAIGIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Honorary Physician to the Royal Infirmary.

IN the year 1818, Professor Puchelt of Heidelberg stated that inflammation of the VENA PORTÆ was a circumstance so rare, that he knew no example of its occurrence. Since that time, thirty-two years have elapsed; during that period, much information upon the morbid conditions liable to affect the veins has been collected; and among lesions of this system, several instances of lesions of the VENA PORTÆ have been observed and recorded. To the principal examples of this lesion reference has been made by M. Raikem in the observations suggested by the case which occurred in his hospital practice.

In the summer of 1841, I had occasion to observe, in the person of a man under my care, at the Royal Infirmary, an example of occlusion of the VENA PORTÆ and its principal afferent veins, which presented several points of interest. Though I have mentioned, in general terms, the circumstances of this case, both in lectures, and in the second edition of my Elements of Pathological Anatomy,* no detailed account of it has yet been published. It was my intention to have published it, with accounts of some of the most important examples of the lesion recorded by Baczynsky, Mohr, Messow, and Lambron. This, however, various circumstances have concurred to prevent. But, as in the preceding memoir M. Raikem has given, in connection with the case recorded by him, a general view of the subject, the present seems a suitable occasion on which to give a short notice of a case which bears, in various circumstances, a resemblance to that recorded by M. Raikem, differs from that case in other points, yet in all may tend to illustrate the effects of a lesion so considerable, as occlusion of one of the large abdominal venous canals.

Charles Macfadyen, aged 32 years, was admitted into the Royal Infirmary on the 2d April 1841, with symptoms of askites. He stated that about two years previous to the date of admission, he had first observed in the left side of the abdomen a swelling about the size of the fist; but that it produced no uneasiness. Not long after this, he was attacked by profuse discharge of blood from the stomach; and this was followed by distension and enlargement of the abdomen. Under treatment these symptoms subsided temporarily; but they recurred two several times after their first appearance; and latterly the swelling of the abdomen has been steadily increasing, without any indications of again disappearing.

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* Elements, &c., p. 127.

When admitted, the countenance appeared pale and colourless, though without any expression of much uneasiness. The features were round, and bore no marks of emaciation. The surface of the person was also pale and bloodless. The abdomen was greatly distended, and communicated a distinct sense of fluctuation. In the left side of the upper part of the abdomen, close under the margin of the ribs, a hard resisting body could be recognised by manipulation. On the right side, no unusual dulness or swelling could be detected.

The pulse was 80, not strong; but the skin had a hot uneasy feeling. The patient had some cough, with expectoration of frothy mucus; and he complained much of difficult breathing. Some bronchial wheezing was heard in both sides; but no indications were perceived of any active disease within the chest.

The urine was not coagulable; but was scanty and high-coloured, throwing down, on cooling, a reddish precipitate.

The patient was subjected to treatment calculated to act on the intra-peritoneal effusion.

The bowels were opened and regulated by the employment of laxative medicines. Diuretics were administered; and blood was drawn from the arm. The use of these measures was followed by temporary relief to the breathlessness; and by an increased secretion of urine. The abdominal enlargement appeared to be at first stationary, that is, not increasing; but eventually the swelling impeded respiration so much, that, on the 28th of May 1841, it was deemed expedient to perform the operation of parakentesis. A large quantity of fluid was withdrawn, with great relief to the breathing for several weeks; and the patient felt himself much better, and was able to move about the ward.

During the whole of the month of June, and the greater part of July, the patient felt himself much better. The abdominal swelling though returning, was doing so slowly; and though the complexion was still pale and sickly, he allowed that his strength was rather increased, and that in general feelings he was more comfortable. During this time he took solution of hydriodate of potass, bitartrate of potass, and afterwards nitrate of potass; while counter-irritating remedies and friction over the abdomen, by means of iodine ointment and other applications, were employed, according as the strength of the patient allowed. The effect of these measures was, that the urine was increased in quantity, and no longer threw down the red sediment, except upon rare occasions. Attention was also given to the regulation of the diet.

About the end of July, however, the abdominal swelling evinced indications of returning; and, when once commenced, the swelling continued steadily to increase. Again, the respiration became oppressed; and the patient was less able to move about. The various means for alleviating his sufferings were continued. But during the first week of August the breathing was so laborious, that it was again thought necessary to have recourse to operation. This was accordingly done on the 8th of August 1841, with the effect of removing a large quantity of fluid, and affording temporary relief.

This second operation was followed by some pain of the abdomen, extending from the wound. For relieving this, leeches were applied with good effect. During the ensuing two weeks the patient seemed to be again recovering. But during the last week of August his weakness increased, and his strength gradually declined till the 4th day of September, when death took place.

The body was inspected on the 5th September 1841; and the following facts were then ascertained.

Five hundred and sixty-four ounces of fluid, of a straw colour, with flakes of lymph, were withdrawn from the cavity of the peritonæum. Several masses of soft lymph adhered to the inner surface of the peritonæum around the wound made by the troisquart.

The liver was greatly diminished in size, being not more than one-third of its usual magnitude. The whole organ was much altered in shape, being rounded and contracted from before backwards, and transversely from each margin to the centre. The left lobe had almost completely disappeared; the only remainder of it being a small portion, shrunk and contracted to not more than the size of a small egg. The right lobe was also smaller than might have been expected, and was evidently shrunk within its normal demensions.

[The surface of the liver was irregularly nodulated, and of a slightly yellow colour. The interior presented irregular masses of greyish, dense, smooth substance, with small yellow masses intermixed with it.] [Hypertrophy of the cellular tissue.]

The principal peculiarity was found in the Portal Vein, and the veins which form this vessel. The Portal Vein was found to be completely filled and distended by firm, yet compressible, elastic matter, as if the vessel had been injected, so that its transverse diameter was fully one inch. The length of this vessel, from the junction of the splenic, superior, and inferior mesenteric arteries to its entrance into the liver, was two inches. At the junction with the liver, it seemed to undergo slight contraction, but was still very large and distended.

When this part of the vessel, forming what is named the abdominal portion of the VENA PORTÆ, was laid open, it was found to be filled to distension with pretty firm, compressible, elastic, dark-brown-coloured matter, which was manifestly coagulated blood. This adhered to the coats of the vessel, which through this space also appeared to be firm, and rather thicker than usual. In that portion of the VENA PORTÆ, which was lodged in the liver, the coagulated matter was firm, but rather lighter in colour, and seemed to be undergoing the process of decolorization. It was quite solid, and was accurately moulded on the interior cavity here formed by the vessel. Two central portions were whitish, and almost like cheese. They showed an irregular laminated structure. The coagulated matter, as thus described, extended upwards into the liver, about one inch and a-half at least, following the course of the first divisions of the VENA PORTÆ HEPATICA.

At one part, where the posterior wall of the Portal Vein was laid open, the contained clots presented, extending along the vessel, an irregular cavity, which appeared to be the only channel by which blood could be conveyed from the abdominal viscera to the liver. This irregular channel was traced a little downwards, and appeared to extend, in a very imperfect and contracted form, into the vessels presently to be described, especially the splenic vein.

The first vessel that contributed to form the VENA PORTÆ HEPATICA was a tube about four lines or more in diameter, which, about one inch below its junction with the VENA PORTÆ, was formed from, or divided into other two vessels. This, which seemed to be the superior mesenteric vein, was filled by the coagulated matter to the point at which its two component veins unite. This vessel was situate at the right side of the portal vein, and joined that vessel on the right. The component veins were pervious; but their coats appeared to be thicker and firmer than usual.

Immediately below the point now indicated, which was about two inches from the junction of the PORTAL VEIN with the liver, the trunk of the PORTAL VEIN underwent as it were a bulging enlargement, most uneven and rounded on the right side and the inferior or sacral aspect, yet sufficiently large also on the left side. This bulging portion was entirely filled with elastic compressible matter, as the portion above it.

At its inferior convex surface it was joined by a large vessel, between four and five lines in diameter, and which was completely filled by the same compressible elastic matter, for the space of two inches and a half at least. This vessel, which corresponded to the inferior mesenteric vein, made a slight bend to the right. It was not uniformly distended by the contained matter; but it appeared more contracted at the point where it forms the portal vein, than below; then about one inch down it appeared to undergo another contraction; then became enlarged, and again was contracted.

At the left of the bulging rounded portion already mentioned was seen the largest vessel of the three, varying from about five lines, or half an inch, to three fourths of one inch, in diameter, filled and distended by the contained matter. The course of the vessel was from left to right, for about three inches at least, not straight, but bent and sinuous. This was the splenic vein. About three fourths of one inch from the point where it joined the inferior mesenteric vein, it underwent a slight contraction in its circumference; but was immediately enlarged to about three-fourths of one inch in diameter. At the point of contraction now mentioned, it was not filled so completely with coagulable matter as at its other portions, which contained the same kind of elastic compressible dark-brown coloured matter as the other parts and afferents of the Portal Vein. Traced from the spleen, the course of this vessel was first downwards, then a flexure or bend, then obliquely upwards and towards the right, where, by its union with the inferior mesenteric vein, it formed the large trunk of the abdominal Portal Vein.

The coats of this vein, the splenic, appeared to be thick and firm.

This vein being less completely filled with coagulated matter, presented at certain points an interior cavity or canal, very irregular. It seemed to be the opinion of one of the observers that this went the whole length of the vessel. It may have done so. But the point was not ascertained; as it was deemed inexpedient to lay open the entire tract of the splenic vein and portal vein. Judging from the points at which the portal vein and the splenic vein were laid open, and from the external feeling of the other two vessels, it seemed impossible to say that there was a continuous pervious channel.

The substance of the liver was of a pale fawn colour, and seemed quite uniform except around the sinus of the vessels, *(Sinus Portarum,)* and the Capsule of Glisson, where the cellular tissue was contracted and indurated.

The section of the right lobe showed the area of several vessels, varying in size, filled and occluded with fibrinous grey coloured matter. One of these, of considerable size, was situate on the superior and posterior part of the large trunk of the Portal Vein, and was about between three and four lines in diameter.

The spleen was much increased in size, and weighed about two pounds; and its substance was firm and fleshy, and of a uniform dark-red colour. The free margin was divided by notches into several slips.

The vena cava was found in its normal state at the posterior margin of the liver.

The state of the hepatic artery was not ascertained.

The thoracic viscera were in a state of comparative soundness.

The foregoing account, excepting the part contained within brackets, was taken from the appearance which the parts presented;

and was afterwards verified, by comparison with the preparation, which is preserved in the Museum of the University, at two several times; first on Thursday the 25th November 1841, and again on Wednesday the 6th of March 1850.

The statement contained within brackets is from the account preserved in the Pathological Register of the Royal Infirmary by the gentleman who had charge of the register. The statement there made would lead it to be inferred that the liver was in a state of *Kirrhosis.* This I did not observe at the time, nor have I been able, when recently examining the preparation, to perceive anything like the appearance of small yellow masses intermixed with irregular grey masses. I mention this discordance, but I am unable to account for it.

Soon after the patient was admitted, the diagnosis which I gave of the case was, that it was an example of *Kirrhosis*, with probable enlargement of the spleen. The reasons for this opinion were various. The first was, because, not feeling any prominent tumour in the region of the liver, yet learning from the history of the case that repeated attacks of hæmatemesis had taken place, and observing the obstinate character of the peritonæal effusion, I inferred that, as the liver could not be felt enlarged, its circulating apparatus must have been obstructed, and that it was in the shrunk or advanced stage of *Kirrhosis.* By one symptom, indeed, this diagnostic inference was opposed. In general the complexion in Kirrhosis is dingy, sallow, or brownish, and not pale and colourless as this man's complexion was. That, however, I thought might be adventitious, and produced by the copious discharge of blood which the patient represented he had suffered, as also probably by other causes. Then there was a complication in the circumstance of palpable enlargement of the spleen, which might tend to modify the symptoms, and especially the colour of the patient's countenance. Another reason on which this opinion was founded was the presence of askites of an obstinate and intractable character. It is well known that, though effusion within the peritonæum is liable to take place in connection with several affections of the liver, yet by far the most obstinate and incurable form is that which takes place along with, or in consequence of, Kirrhosis. In simple enlargement of the liver, the fluid invariably diminishes, and eventually disappears as the enlarged gland returns to its natural size. In hypertrophy, also, which has not attained a very confirmed stage, that is, where the liver is still capable of being reduced in size under the use of proper dietetic and remedial measures, the dropsical effusion within the abdomen may in like manner, and at the same rate, be made to disappear. Very different, however, is the case with the kírrhotic or orange tuberculated liver, of which it has hitherto been ascertained, that, as no remedies make any impression on the morbid state of the *akini*, so none make any impression on the circulation within the gland, and the amount of fluid effused within the cavity of the peritonæum. It appeared, therefore, natural to conclude conjoining the obstinate, yet local, character of the dropsical effusion, with the fact that the liver could not be felt to be enlarged, that the organ was in that peculiar morbid condition called *Kirrhosis*.

The reason for thinking that the spleen was enlarged was sufficiently obvious; a swelling being found to exist exactly in the situation of that organ, which could be referred only to the spleen, the left side of the omentum, or some new growth developed in that region.

When the patient died, and the body was submitted to examination, only part of the diagnosis was found to be confirmed by the appearances presented. The spleen was indeed found enlarged, as had been predicted; the liver also was found to be unusually small and contracted; and if mere external configuration and induration and contraction of the cellular tissue of the gland, were sufficient to constitute Kirrhosis, that part also of the diognosis might be allowed to be confirmed. Indeed, according to the views of the gentleman who recorded the appearances in the Register, it seemed to be regarded as an example of *Kirrhosis*. In this inference, however, I was unable to acquiesce. The hepatic substance did not present those characters which I regard as essential to constitute Kirrhosis. It was of a uniform fawn colour, and presented not that internal granular appearance which experienced observers are aware belongs to *Kirrhosis*.

When, on the other hand, we found the liver so much diminished in size, so greatly altered in shape, and one lobe reduced, as it were, to a mere rudimental fragment, and a lesion so extensive and complete, as obstruction of the principal part of its circulating vessels, it was easy to see that changes even greater than those taking place in Kirrhosis had been effected, and that these changes must have exerted a very great influence both on the hepatic circulation and secretion, and on the circulation of the abdominal organs in general.

I shall endeavour to form some estimate of the nature and effects of these changes in the subsequent observations.

In many respects this case affords a very remarkable example of obstruction of the VENA PORTÆ. The obstruction was very complete, affecting at once the VENA PORTÆ in the liver, the abdominal VENA PORTÆ, and the three principal venous tubes by the union of which that trunk is formed. The channel of the vessel also was as nearly obliterated as was possible to be; for the vessel and its afferent veins were filled to distension by clots of blood; and though a small irregular shaped longitudinal cavity was observed within the abdominal portal vein, and part of the splenic vein, the cavity of the hepatic portal vein was completely filled and obliterated. No blood, therefore, could have by this channel been conveyed into the liver.

In inquiring into the cause of this obstruction of the PORTAL VEIN by coagulation of blood, we are met by various difficulties. It is manifest that the obstruction had not been of recent origin, and all the symptoms of the case concurred to show that it must have been of one year, probably nearly of two years' duration or longer. The history of the case does not present any set of well-marked symptoms which can be taken to denote the time, at which the obstruction took place, the time at which it commenced, the manner in which it proceeded, rapidly or slowly, and the date of its completion. All that can be said is, that it had probably taken place before the first attack of hæmatemesis; and it seems that it is not an unreasonable inference to conclude that, when the effusion had taken place within the peritonæum, the obstruction of the Portal Vein must either have been far advanced, or nearly as complete as it was found to be after the patient died.

A question not less difficult is to determine whether this obstruction was the consequence of inflammation, attacking either the PORTAL VEIN in the liver, or the abdominal PORTAL VEIN, or of some other cause retarding and impeding the motion of blood within the vein. The obstruction appeared most complete, and the clots most decolorized, in the former vessel. In the latter, and in the afferent vessels forming the abdominal part of the vein, the clots were darker coloured, more friable, less tenacious, and presented appearances more nearly approaching to the characters of coagulated blood. The tunies of the vein were certainly firmer than natural, and those of the splenic vein especially seemed thickened and indurated, as if they had been the seat of inflammation. It is well known that the coats of the PORTAL VEIN are firmer than those of other veins, and approach in consistence the characters of arterial vessels, so that it becomes not easy to say, to what extent the firmness in the Portal Vein was a proof of the previous existence of an inflammatory process.

One circumstance which deserves attention in estimating the evidence for this view, is the state of the cellular tissue about the sinus and the capsule of Glisson. This was, as it were, condensed, contracted, and slightly more tough than natural; and it might without violence have been inferred that this part was originally the seat of some form of congestion, irritation, and inflammation. It is known that this is particularly observed to take place in the liver affected by *Kirrhosis;* and, from some expressions in the description in the Register, it might be inferred that a similar change had taken place. The change, I allow, had taken place so far as the cellular tissue connecting the vessels with the liver, and forming the capsule, was concerned. But, on the other hand, nothing like the essential characters of the true Kirrhotic liver, nothing like the peculiar granular or tubercular bodies, constituting that form of structure, was observed. In short, I think it may be inferred from the phenomena presented by this liver, that the contracted rounded figure, and the contracted indurated aspect of the cellular tissue, is not peculiar to *Kirrhosis*, or characteristic of that affection, but common to it with other morbid states of the liver. It was a conclusive example to show that hypertrophy of the cellular tissue, as it is commonly named, does not constitute the essential pathological character of *Kirrhosis*.

In other respects, it may be allowed that this state of the subhepatic cellular tissue was the effect of some degree of previous inflammatory action. But whether this had been the cause of the obstruction, or a concurrent and simultaneous effect, it is manifestly impossible, if we adhere strictly to facts and observation, to determine.

If, however, we admit the circumstance of there having been a process of inflammation somewhere about the Capsule of Glisson or the SINUS PORTARUM, and if this inflammatory process extending to the VENA PORTAE had affected its coats to such a degree as to cause roughening of the inner coat, and effusion of lymph within that vessel in the liver, it is not difficult to understand how the obstruction of the interior of the vessel and its afferent veins took place. If the coats of the Portal Vein in the sinus,—where, it must be remembered, the obstruction was most complete, and the contained clots were most completely decolorized,—had become so much inflamed as to pour out lymph along the interior of the vessel for half an inch, this must have caused the blood to coagulate over the lymph, and thus formed an impediment to the movement of the blood from the abdominal part of the vessel to the hepatic portion. When this impediment had once begun to be formed, it would increase every hour until it was either swept onward by the force of the current of blood, or filled up completely the calibre of the vessel. The former result is of no avail in relieving the affected portion of vein in the case of the VENA PORTAE, because the blood is carried in that vessel from trunks to branches and small vessels; and the mass of lymph and coagulated blood formed within the trunk would only block up more completely any smaller vessel or vessels into which it is carried. This result therefore terminates in the second; namely, that when an impediment to the onward motion of the blood is once established, the blood behind, being stopped in its progress, becomes coagulated, first in the abdominal portion of the VENA PORTAE, and then in one or more of the afferent vessels which go to form that large venous trunk. In this manner, therefore, it is easy to see that, if an impediment to the upward movement of the blood were formed in the hepatic division of the Portal Vein, that impediment must, apparently of necessity, cause obstruction to the movement of the blood, first, in the abdominal division of the Vein, and afterwards in the afferent veins which unite to form this vessel. It is perhaps some confirmation of this mode of viewing the origin and progress of the obstruction, that in the upper or hepatic division the obstructing material was either lymph or decolorized blood, firm, compact, and lamellated; while, in the afferent vessels, and partly in the lower or abdominal division of the Portal Vein, the obstructing material was softer, more friable and loose, less compact, more nearly resembling coagulated blood, and appeared on the whole to be more recent in origin.

A question may here be proposed, namely, how does it happen that in this instance the obstruction, if of inflammatory origin did not prove fatal, at the time at which the vein was in a state of inflammation? Inflammation of a vein is in general a fatal disease; in few instances does the patient survive the attack; and if he do survive, he dies of the secondary effects.

The difficulty of answering this question in a satisfactory manner, I pretend not to under-rate. The following facts and considerations, nevertheless, may enable the pathological inquirer to make some approach to the explanation of the difficulty.

There are two or more forms of venous inflammation, exactly as there are more than one form of inflammation of other tissues. The two more common, are, first, that which is attended with effusion of plastic lymph alone; and, secondly, that which terminates in suppuration. The former does not in all instances terminate fatally immediately. It appears, indeed, to be a less fatal form of the disease than the suppurative *phlebitis*, and certainly it gives rise to constitutional symptoms, much less intense and violent. Often even it appears to be a chronic form of disease, and though bad enough in this respect, that it proceeds steadily to fill up or at least contract and diminish greatly the channel of the vessel, yet it neither so perfectly and speedily contaminates the whole current of blood, and affects the whole venous system, nor does it give rise to the well-marked and intense disorder of the system at large, which invariably are the effect of suppurative *Phlebitis*.

If, therefore, we are to admit that this obstruction or occlusion of the Portal Vein was the effect of inflammation, either originating in the vessel, or extending from without to its tunics, it seems most natural, and most consistent with what we know of this disease in other circumstances, to infer, that it must have been the plastic or lymph-effusing, and not the suppurating form of the disorder.

It is, nevertheless, necessary to advert to another circumstance in examining the mode in which this obstruction has arisen. It happens, perhaps not very often, yet occasionally, that a peculiar affection of the venous system, with the appearances and effects of obstruction and occlusion of many venous canals, takes place in connection with enlargement or hypertrophy of the spleen. This I have seen take place in two instances, in which all the phenomena were observed with the greatest care. In the first of these instances, the symptoms attending affection of the venous system were so carefully observed, and the whole course of the case was so accurately watched, that while they admitted of exact comparison with the appearances found in the organs of the dead body, I was enabled to apply the knowledge thus obtained, in predicting the course and termination of the symptoms after they had become developed, in the second case. Nor was I mistaken. For in both cases, in the second as in the first instance, it was found after death, that, in the veins of the head, chest, and abdomen, there had taken place a peculiar affection, in which the blood in the venous canals had become stagnant, and adherent to the venous walls, was mingled with lymph and purulent matter, and the coats were, if not much thickened, at least in certain points rendered rough interiorly.*

The connection between this species of general *Phlebitis*, and hypertrophy of the spleen, I do not pretend to explain. The fact of the connection is all that I think can be established as a principle in pathological science; and for all useful purposes, the fact of this connection is quite sufficient.

Now it is to be observed, that in the instance of the patient whose history is given in this Memoir, the fact of enlargement or hypertrophy of the spleen was ascertained during life; and, so far as the history of the case can be trusted, the morbid enlargement of this organ had taken place at an early period of the disease, which terminated the life of the patient. It is highly probable that enlargement of the spleen had existed for two years, and commenced some time before the most urgent symptoms of the patient's disorder had made their appearance. If, therefore, this be admitted, it may also be allowed that the hypertrophied state of the spleen might in this instance, as in the two others, have given rise to an affection of the venous system, of the nature of

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* Two cases of disease and enlargement of the spleen, in which death took place from the presence of purulent matter in the blood. I. Case of disease in which death took place in consequence of the presence of purulent matter in the blood. By David Craigie, M.D. &c. Edinburgh Medical and Surgical Journal, Vol. lxiv. p. 400. Edinburgh, 1845.

inflammation of the internal coat, and certainly causing the formation of lymph and clots of blood in the interior of the vessels. This view does not essentially alter the question as to the actual cause of obstruction of the Portal Vein, neither does it necessarily remove that cause from the category of venous inflammation. It merely refers the inflammation of the Portal Vein to another initial point, a different etiological agent. I do not here undertake to say, that hypertrophy of the spleen actually induced the deposition of lymph and coagulation of the blood within the Portal Vein. I do not propose the evidence for such an inference as complete. All that I think can be established under the circumstances, is the fact of a connection between hypertrophy of the spleen on the one hand, and such affections of the venous system on the other; that lymph and purulent matter may be conveyed into certain parts of the venous system, and may thereby stop the movement of the blood, cause coagulation of that fluid, and occlusion of those venous tubes in which these changes take place. It would be an omission in the inquiry to overlook the fact of the connection of hypertrophy of the spleen with affections of the venous system, or not to allow the apparent influence of the spleen upon the circulation of the venous system, to be in the state of a subject open to further investigation. I wish not, however, to invest this connection with undue importance; and, in order to ascertain the true amount of this importance, after directing attention to the facts, I leave it to those whose opportunities may enable them to consider it more fully.

It might by some be imagined that the enlargement of the spleen was an event subsequent to the occlusion of the Portal Vein; and that the latter circumstance was the cause of the morbid enlargement of the spleen. This appears not to be a very probable inference; but, as I have no means either in the way of fact or argument to determine the question in the one way or the other, I refrain from any observation on it.

The effects of the occlusion of the VENA PORTÆ are here witnessed in a very distinct manner.

The most remarkable of these was the atrophy or shrunk and diminished state of the liver.

This atrophy had advanced to a very great degree. From some accident the liver was not weighed; but its size was certainly not more than one third of the average size of the liver in adult males. The left lobe was entirely gone, excepting a small oblong pyriform mass about two inches or two inches and one-fourth long, and not more than half an inch broad. The right lobe was also much diminished in size, probably about one-half the original bulk, or not quite so much.

All this diminution in size, and the corresponding alteration in shape, it seems natural to ascribe to the occluded state of the Portal Vein, and the consequent diminished supply of blood to the gland. This diminished supply acted palpably on the nutrition and growth of the organ, and, no doubt, upon its secretion. M. Raikem has adverted to the notion so much favoured by various physiological speculators, that the hepatic artery is employed in maintaining the nutrition of the liver, while to the Portal Vein belongs the function of conveying to the gland the materials from which bile is to be prepared; and has adduced, to show its incompetency, several conclusive arguments. It is scarcely possible to conceive a stronger argument against it than is furnished by the facts of this case. The Portal Vein was completely obstructed, and no blood must for a long time have been conveyed through its branches into the gland. The liver is likewise very much reduced in size, not, indeed, uniformly and equally in all its parts, but still so much and so generally atrophied, that it is difficult to ascribe the diminution and wasting of parts to any other cause. The two circumstances, therefore, appear to stand in the relation of cause and effect.

It must be admitted that, as the state of the hepatic artery was not ascertained, and as this tissue might have been occluded by the pressure of the Portal Vein, it may be objected that this atrophy was owing as much to occlusion of the hepatic artery as to that of the Portal Vein. It appears to me, nevertheless, that this cannot form a very strong objection, nor would impair much the force of the argument; because, so long as the great supply of blood by the occlusion of the great channel of its conveyance is cut off, the small amount that could reach the gland by the hepatic artery must have had no great influence.

Regarding the secretion of bile, not much was ascertained. A small quantity of light-coloured bile was found within the gall-bladder; and during life the alvine discharges were of the usual colour. But the function on which the lesion appeared to exercise most influence was that of *Hæmatosis* or sanguification. The pale anæmic aspect of the patient was probably the most characteristic of the external symptoms of the disease. Blood evidently was not prepared in the normal manner, and did not contain the proper proportion of red particles; or, if prepared, it was not distributed to the surface, which was everywhere colourless, and like that of a person who had suffered from hemorrhage. It would be refining unnecessarily upon this and similar symptoms, were we to consider them as independent of, and unconnected with, the deficient supply of blood to the liver, the defective secretion of that gland, and the consequent lesion in that part of the process of nutrition which Dr Prout has denominated Primary Assimilation.

On the subject of the effusion within the *Peritonæum* as an effect of the occlusion, it is unnecessary to make any remark. In this instance the effusion had probably two causes; the contracted state of the liver and its capillary circulation, and the obstructed channel of its largest blood-vessel.

On the habits of this patient, circumstances prevented me from obtaining any information that could elucidate the origin of the disorder. The man, though from Ireland, was above the rank of a common labourer or workman, and, I think, pursued some one of those lighter avocations by which the natives of that country, when from home, endeavour to amend their condition. He was not intemperate in habits, that is, he was not very much addicted to drinking. Indeed, he was rather a good specimen of an industrious and well-behaved man, and seemed anxious to do everything that was thought necessary to re-establish his health. Whether he had ever undergone ague, or had been exposed to its power, could not be ascertained.

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *A Popular Treatise on the Kidney, its hitherto unknown Functions, and its Diseases, in connection with the Circulating Animal Oils, &c.; with Advice to Persons in their Secretions.* By GEORGE CORFE. London, 1839. 8vo, pp. 304.

2. *The Physiognomy of Diseases.* By GEORGE CORFE, M.D., Resident Medical Officer at the Middlesex Hospital, &c. London, 1849. 4to, pp. 151.

IN the sixty-eighth volume of this Journal,* after giving an account of the researches made by Muller, Bowman, Reichert, Gerlach, Bidder, Kolliker, and Toynbee, on the minute structure of the kidney, was subjoined at the same time a short statement of the deductions formed from microscopical examination by Mr Toynbee and Dr Johnson, on the true nature of Granular Disease of the Kidney. It was then shown, that Dr Johnson maintained, that the change of structure, to which the name of Granular Disease of the Kidney is applied, consists in the deposition of fatty matter in the cortical texture of the gland, in an exaggeration or increased amount, in short, of the fatty matter which exists naturally in small quantity in the epithelial cells of the healthy organ.

Similar also are the representations made by Mr Toynbee, with this exception, that while Dr Johnson represents the investment of the Malpighian plexus to be very rarely affected by this oleaginous infiltration, and the Malpighian vessels to escape disorganization, Mr Toynbee states that in the early stage of the disease the *corpora globosa* are dilated, their arteries are enlarged, and their capillaries are distended and varicose; that, in the second stage, the tuft is broken up; and that, in the third stage, their vessels are so much altered that they are incapable of being injected. They appear to be compressed and obliterated by the

* Edinburgh Medical and Surgical Journal, Vol. lxviii. pp. 449 and 480. Edinburgh, 1847.

contraction and induration of the cortical portion. According to both authors, the tubules of the cortical portion contain many oil globules, blood globules, and more or less granular matter.

Though this explanation of the phenomena of granular disease was not entirely new, yet it afforded important corroborative evidence in confirmation of the results obtained by various other observers. Thus Dr Gulliver showed in 1843, that in kidneys affected by granular disease, fatty globules and crystalline plates of cholesterine can be seen by the microscope; and Dr Davy obtained from them by chemical analysis, margarine, cholesterine, and a trace of oleine. Gluge also showed, from microscopical examination of kidneys in this state, that the infiltrated matter is in general oil or fat, and applied to it the name of STEAROSIS, in order to designate the peculiar species of transformation. We have only on this point to observe, that the name should be STEATOSIS. Similar views had been given about the same time by Vogel, Canstatt, and Hechte. It must nevertheless be allowed, that the researches of Dr Johnson and Mr Toynbee possess very great merit, because, though not the very earliest, they give probably the most complete accounts of this peculiar form of transformation, and in all probability they will always be appealed to as affording evidence of the fact, that the kidney is liable to this interstitial penetration of fatty and oleaginous matter, and is thereby rendered unfit to perform its proper functions.

After making these preliminary statements, it may perhaps appear to be making too large a demand upon the indulgence of readers, if we say that it appears, that not even the inquirers above mentioned are entitled to be regarded as the earliest promulgators of this doctrine. That this, nevertheless, is the fact will appear from the following statements.

In 1839, Mr George Corfe published the Popular Treatise on the Kidney above mentioned, in which that gentleman professed to explain the functions and diseases of these glands, in connection with the animal oils, their uses, and their morbid conditions. The work, which is divided into ten parts, contains a considerable proportion of matter, upon the merits of which and its connection with the proper subject of the volume, different opinions were then and probably will continue to be entertained. But under the head of the Anatomy of the kidneys in part third, that of the blood in part fourth, and that of the Circulating Animal Oils in part fifth, were given various statements and representations which were, to say the least of them, new and striking, and which, if they were verified and corroborated, were certainly entitled to attention. Attention, nevertheless, of a general nature, they could not be said to receive. The views given were scarcely noticed in this country, at least immediately; and abroad they appear to have been known to not more than one author of much note. The only person, almost, who adverted to the statements of Mr Corfe, was Mr Solly, who, in his translation of the work of John Muller on the glands, gave an abstract of the principal facts which Mr Corfe conceived himself to have established. This was done in such a manner, that it ought to have directed the attention of readers to the work itself. But, so far as we are informed, it has not fully succeeded in this result. A testimony not less honourable to the researches of Mr Corfe, is that his book is mentioned by Gluge, among those treatises and writings which that diligent observer enumerated as worthy of being known in the literature of the diseases of the kidney.

These circumstances, however, do not constitute the sole claim which the treatise of Mr Corfe possesses on the attention of the profession. Since the researches of Gulliver, Davy, Gluge, Canstatt, Johnson, and Toynbee, as well as other inquirers subsequent to these, have shown that one or two forms at least of granular kidney depend on the infiltration of the tubules with fatty or oily matter, the representations of Mr Corfe have acquired a new degree of interest and importance. To that gentleman belongs the merit, whatever it may be, of first propounding the doctrine, that granular degeneration of the kidney depends on, and is connected with, the infiltration of its tubules with fat. As the author was led to this inference by other observations and reasonings, and as his work contains several views, physiological and pathological, which promise to elucidate various morbid states of the system not very well understood, justice seems to require that we direct attention to those parts which seem most important.

If, however, it be reasonable, that to researches and doctrines which were first made known eleven years ago be assigned their proper value, it becomes so much more requisite at the present time, when Mr Corfe places before the profession another work in which, though of a more comprehensive character, the doctrines peculiar to the author are again brought forward and enforced with additional arguments. In the work entitled "Physiognomy of Diseases," Mr Corfe considers the physiognomical expression of persons labouring under almost every disease liable to affect the organs of the head, chest, and abdomen, and collects with considerable judgment those characters by which the practised observer is often enabled to tell rapidly and with comparatively little examination, not only the disease under which a patient labours, but the form and the degree of the affection, and to estimate the probable chances of recovery or of termination in death. Among others, diseases of the kidney come in for a considerable share of attention; and the author takes occasion to expand his views, and present them, on the whole, in a more distinct and intelligible form than that in which they appeared in his first publication.

It shall be our endeavour to present a condensed view of the physiological and pathological doctrines of the author, so far as they can be distinguished from those of a different character contained in the work; and, in so doing, we avoid, as much as may be practicable, repeating the information already given in volume sixty-eighth, and the first part of the present volume.

In the third, fourth, and fifth chapters of part first, Mr Corfe takes a view of the researches of modern anatomists on the structure of the kidney, beginning with Malpighi and Ruysch, and adverting in order to the statements of Bellini, Bertin, Ferrein, Eysenhardt, Muller, Doellinger, and Huschke. In the course of this survey, the author adverts particularly to a net-work of filaments, vessels, or minute tubes rather, which were first described by Bellini,* and afterwards more minutely by Eysenhardt, and which that anatomist regarded as urinary ducts. These filaments, ducts, or minute tubes, are smaller than the urinary ducts. They may be injected both from arteries and veins; and according to Mr Corfe, while Eysenhardt was wrong as to their being urinary ducts, he had some reason, from the representation that they proceeded from the minute veins. Muller has rather severely criticised the inferences of Eysenhardt as to these ducts being urinary tubes. Mr Corfe again criticises Muller's views. This net-work of minute tubes contains, he alleges, A SET OF VESSELS HITHERTO UNRECOGNIZED. These vessels, it afterwards appears, are OIL-TUBES.

These second sets or branches of ducts, which are described by Weber and Muller as branches of the first set of ducts, are undoubtedly the same as those adverted to by Ferrein, Bertin, Eysenhardt, and Doellinger; but which are not ducts at all in the sense of which Muller speaks of them. They have, he repeats, a separate and distinct office, hitherto unknown, and consequently unexplained. It is to him clear that the wonderful network, as anatomists term them, or set of blood-vessels, which lie about, between, and around the urinary serpentine or cortical ducts, has not received the attention to which from their importance they are entitled.

After all the research made on the minute structure of the kidney, and especially on the nature of the connection between the blood-vessels and the secreting ducts, the following points he thinks are left undecided.

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* We are not quite sure that the author does not speak rather in a confused manner about these tubes. He speaks as if he regarded the tubes of Bellini and those described by Eysenhardt as the same. The tubes of Bellini, or the Bellinian ducts, are the straight tubes visible in the medullary cones; and these appear to be altogether excretory. The net-work seen by Eysenhardt is in the cortical matter.

1. That the arteries of the kidneys have no direct union with ducts.

2. That the Malpighian bodies do not communicate in the slightest manner with ducts.

3. That these ducts twist and fold one over the other, to an indefinite length, terminate on the surface in blind ends, and are uniform and unchanged in diameter throughout.

All this, it is to be remembered, is given in 1839.

Against the conclusions now stated, Mr Corfe urges the objection that all the ducts injected by the air-pump from the ureter are *not* urinary ducts. One set of the ducts thus demonstrated is for a particular purpose to be presently explained.

Mr Corfe next states his ideas regarding the nature and office of the kidney. First, he infers from the position of this gland, in the midst of a large and thick mass of fat, that its nature is to act in the highest degree of animal heat, and that without this condition it does not properly perform its functions. Secondly, he infers that the office of the kidney is primarily to receive oil into its interior, for the purpose of purifying that oil, and of lubricating the parts, and for receiving from that oil DRAINS, or secretions as they are called, in order to discharge these secretions in an excrementitial form from the system. A secondary or subordinate office of the kidney the author further maintains to be to receive the DRAINS or secretions from the venous blood, not from the arterial blood, as is universally supposed. And, in the third place, Mr Corfe maintains that the oil thus purified in the kidney, then passes into the circulation by the venous trunks within the kidney, and circulates through the system, where it performs various important services in maintaining health.

Next comes the illustration of the peculiar structure of the kidney from comparative anatomy.

In the dolphin, says Mr Corfe, the kidney, like most others among fishes; (the dolphin, however, is a mammiferous animal, not a fish); consists of an assemblage of small lobes, situate on the branches of the ureter; each lobe being composed of small round lobules, joined in tufts, and seated like clusters upon the ureters. On one side of the lobule is seen an orifice, into which passes the branch of the ureter; and the aperture is surrounded by a small calyx or cup. In each lobule is a separate nipple, the summit of which is surrounded by vascular substance. The lobules consist, as in MAMMALIA, of serpentine, cortical, and straight medullary tubes. The division of each substance in the lobules is similar to that in other classes of animals, in so far as the urinary ducts originating in *fasciculi* or bundles from the nipple, are afterwards collected, become more and more prolonged, spread outward and widely in their serpentine course, and thus form the vascular or cortical substance.

From the small calyx or cup above mentioned, as surrounding the aperture of the renal lobule, come off multiplied minute cylindrical canals, shaped like a feather, and which, therefore, Mr Corfe designates FLOCCULENT FEATHERY OIL TUBES. These allow the oil that has passed up between the layers of the cup and the ureter to pour out, according to the demonstrations of Mr Corfe, its watery secretion into them, and to be mingled with that secretion or drain from the blood found in the urinary ducts. It is only after this commixture that the fluids thus combined constitute urine.

At a subsequent part of the work, Mr Corfe considers the anatomy of the kidney under the following heads:—First, The three membranes of the kidney;—the adipose membrane; the reflected membrane; and the reticulated membrane. Secondly, The Ureter; the Pelvis, with its oil, and its course without and within the gland. Thirdly, The FLOCCULENT FEATHERY OIL Tubes. And lastly, The urinary ducts; the veins; the arteries; the nerves; and the lymphatics.

The adipose or cellular membrane of the kidney is merely the thick cushion of adipose substance in which each kidney is imbedded. It is so well known, that we dwell not upon it. But in the speculations of Mr Corfe, it occupies a most important position, and performs, according to him, a most important function

Enclosed in this cushion, within numerous cells, is contained, according to Mr Corfe, a dense mass of oil, to which he applies, in imitation of the language of the sacred writings, the term LOG OF OIL. The cells most remote from the kidney are the largest, and the most dense of the whole LOG. "As the oil drops from cell to cell, it passes through partitions thinner and thinner, or sieves finer and finer, until the net-work is so delicate, around and within the gland, that it requires a magnifying power to demonstrate its presence." "The oil corresponds in its consistence to the circumference of the cells which contain it. If a small portion of oil is taken out of the adipose membrane most remote from the kidney, and smeared over the hand, it runs lumpy and hard over the warm skin. If a portion, however, be removed from the minute cells, just as it is entering the sinus of the kidney, and similarly treated, it runs over the hand like tallow under the flame of a candle. If the gross lumps be taken and held over a spirit lamp, it sparkles and burns with a crackling noise, as though it contained water and salt. But, on the contrary, a portion from the sinus of the kidney burns silently, rapidly, and is truly PURE OIL."

The proper or reflected membrane is that which immediately invests the kidney. It passes into the bosom or sinus of the gland, being reflected within itself. It consists of two layers;

the outer one being dense and fibrous; the inner one thin; and between them is a delicate net-work of cellular tissue.

The reflected membrane having surrounded the kidney, passes to its mouth (the sinus), and lies on each side of the pelvis, to which it is here intimately united. The membrane then follows the course of the pelvis and its seven branches, the calyces and their branches. The farther course of this membrane through the interior of the kidney the author compares to the hand and fingers when covered by a glove. He then has recourse to the usual comparison adopted by anatomists in describing the serous membranes, and states that it resembles a double-headed nightcap, having the substance of the gland between the two folds of the nightcap.

The reticulated membrane is that which adheres immediately to the kidney, and sustains the blood-vessels, and penetrates into the substance of the kidney between its lobes, but not extending to the medullary portion. It may be compared to the PIA MATER.

The pelvis is usually divided into seven compartments, which have been named CALYCES. These the author denominates the SEVEN SEMICIRCULAR OIL-TUBES of the kidney. From these seven principal oil-tubes proceed other smaller tubes, varying from eighty to one hundred in number, which are also, he maintains, semicircular oil-tubes.

From the whole series of these large and small semicircular oil-tubes proceed an innumerable set of tubes, shaped, according to the author, with barbs and barbules like a feather. These, therefore, the author terms the FLOCCULENT FEATHERY OIL-TUBES of the kidney. They are represented as coming off from the ends of the seven primary tubes, and, when properly prepared for demonstration, hanging about like down or shaggy substance. They are dispersed throughout the whole medullary substance of the gland, and are estimated as composing at least three-fifths of the bulk of the organ. They may be traced to within one quarter of an inch of the surface of the kidney, where they are lost in minute terminations. (P. 67.)

This down-like or flocculent termination of these alleged oil-tubes is represented in various figures in a plate with which the work is illustrated.

From the edges of the pelvis, or rather, as we understand, from the edges of the divisions, proceed, besides the feathery oil-tubes now mentioned, an innumerable set of the same kind of tubes, which, for the sake of distinction, the author designates THE LATERAL FEATHERY OIL-TUBES. These are not so numerous as the first-mentioned tubes; but their office is the same. To give something like an idea of the relative position and appearance of both sets of tubes, the author states that the whole series of feathery oil-tubes might be justly represented by forcing in the ends of the fingers of a glove, a large bunch of white small downy feathers, and by putting some also along the various seams of the glove; when the former would represent the FLOCCULENT OIL-TUBES, and the latter the LATERAL FEATHERY OIL-TUBES.

In shape, these Flocculent Feathery Oil-Tubes are said to be similar to a very fine feather with its barbs and barbules. The barb of these feathery oil-tubes lies upon an urinary duct, or between two of these ducts, as they descend from the surface of the gland. In this manner, they become intimately bound up with the small bundles of urinary ducts. The barbules of these feathery tubes penetrate the sides of the urinary ducts, and enter these ducts obliquely, or in what the author terms a valvular mode, such as the ureter forms in its entrance through the coats of the bladder. This is the reason why they cannot be injected from the urinary ducts, neither can they be injected from the veins or arteries; for they have no connection with the blood-vessels.

These tubes, throughout their entire course, are surrounded by delicate shaggy or downy membrane, which is reticulated, and has the appearance of minute hoar-frost. Under the microscope, it resembles fine lamb's wool, or white moss.

The majority of these Feathery Oil-Tubes pass into the urinary ducts, in the vascular or cortical substance of the gland; and according to the author, they convey away the excrementitious drain from the oil into these ducts. Those, on the other hand, which he designates the Lateral Oil-Tubes, join the urinary ducts in the medullary substance; and he thinks that these, from their relative position, pour out an oleaginous fluid, less watery in its character than that which is drained away from the other feathery oil-tubes, situate in the vascular substance. The use of this oleaginous fluid is, he apprehends, simply to lubricate the internal surface of the pelvis, in the same manner in which the ureter is lubricated by that oil, outside the pelvis and kidney, which passes into the orifices of this duct.

As the feathery oil-tubes are arranged along the sides, or upon the urinary ducts, they form very minute spaces or intervals in which pass the blood-vessels of the gland. Mr Corfe here however states his conviction, that the tunics of these oil-tubes derive more nourishment from the oil contained within them, than from arterial blood flowing without them; in the same manner in which the quill of a fowl is nourished in its barbs and barbules, by the oil which passes up the pith or fine membrane within the quill. He compares its function to that of the marrow within a bone.

The preceding account of these alleged oil-tubes is next applied to the explanation of the structure of the kidney.

That portion which forms the proper substance of the gland consists, we are told, of a collection of innumerable urinary ducts, blood-vessels, and the terminations of the feathery oil-tubes, connected together by delicate reticulated membrane. The origin of each duct commences within the venous circle, at the surface of the gland, and terminates at the papillæ. If the kidney of a person who has died suddenly, while in good health, or that of a healthy sheep, be injected from the veins with red size, and then sliced open, we may observe a peculiar dark red coloured zone, on the whole surface of the gland, about one-fourth or one-sixth of one inch in depth. The injected matter appears to have run more minutely in this than in any other part of the gland. When divided into sections, and viewed under the microscope, this is seen to be the fact.

This appearance, the author states, proceeds from a series of minute vessels which have been injected. These vessels are an innumerable set of hair-like shoots from the renal vein, when it approaches the surface of the gland.

If another healthy kidney be injected in the same manner by the veins, and the reflected membrane be stripped off, and a thin slice be taken from the surface, and dried on clear glass, the observer perceives that within the circular spaces which the vein forms at the surface of the organ, is a set of white or whitish-grey points, larger than the Malpighian bodies on the arteries. Each circular venous space encloses one of these points, which are the blunt ends, or more properly, says the author, the origins of the urinary ducts. These are the white portions which Ferrein mistook for the origins of the white ducts of which he speaks. From the appearances thus presented Mr Corfe draws and establishes the inference, that the urinary ducts originate within these venous circles, and that these origins of the urinary ducts are joined to the veins by minute twigs from these vessels, the whole being connected by reticulated membrane.

Again, he tells his readers, that the blunted origins or caeca of the urinary ducts on the surface of the gland receive from the internal layer of the reflected membrane of the kidney a delicate prolongation, which dips down within each venous circle to pass into the duct; that, consequently, this internal layer of the reflected membrane is continuous with the internal lining of the urinary ducts and that of the pelvis and ureter; that the external layer of the reflected membrane and the internal lining of the ureter, pelvis, semicircular oil-tubes, and feathery oil-tubes, are continuous; and that this continuity of surface takes place at the margins of the semicircular oil-tubes, and along the feathery oil-tubes within the gland.

That all this is very clear and intelligible we are by no means certain, though we have studied to convey the author's meaning, selected from amidst a considerable amount of repetition and iteration upon white ducts, urinary ducts, and oil-tubes. The following summary on certain obscure points must appear therefore in the words of the author.

"The numerous urinary ducts having passed out from the venous circles, run down like rays from the circumference of a circle to its centre, converging towards the semicircular oil-tubes, within which spaces they pass to their points of termination, namely the papillæ. Just before they reach the semicircular oil tubes, their spongy, friable character disappears, and they become dense, fibrous, and hard. This change of character in the urinary ducts arises from two circumstances. They become more and more compact, and are girt together within as small a space as possible, to allow them to pass through to their respective semicircular oil-tubes. They are also bound firmly round by the dense band of substance which composes every semicircular oil-tube."

"But besides this peculiarity in the structure of the urinary ducts of this portion of the gland, they are now joined by the innumerable barbs and barbules of the feathery oil-tubes. The spot where this junction commences may be stated to be mid-way between the semicircular oil-tubes and the surface of the kidney. As the urinary ducts proceed downwards, they are again and again joined by numerous other oil-tubes, until the mass arrives within a minute distance of the semicircular oil-tubes, when a very scanty supply is sent off to the ducts, and the whole collection or assemblage passes between its respective semicircular oil-tubes, and assumes that appearance known as the medullary or tubular structure of the gland. The urinary ducts in their course downward run into one another, so that twelve or fifteen ducts are folded up into two or three at the medullary substance.* But that bifurcation which some authors have spoken of in the urinary ducts is an erroneous supposition. The mistake has originated with them, in looking upon the innumerable feathery oil-tubes which join the urinary ducts, as so many urinary ducts also. These ducts are of the same diameter and of the same admeasurement throughout; they could not therefore bifurcate without suffering a diminution in diameter."†—Pp. 75, 76.

"'It is not therefore,' Mr Corfe continues, 'a bifurcation of the urinary duct, but the adjunction or falling in of an oil-tube with a duct, which renders it (the urinary duct) forked. Hence there is no appreciable difference between the diameter of a urinary duct before the oil-tube pierces and enters it, than after this adjunction takes place. If it really was a bifurcation, as Muller's description implies, then the numerous branches into which the duct splits, would render this duct twelve times larger in diameter at the medullary portion than in the vascular substance; which is wholly opposed to the fact. If ten thousand capillary vessels join into one, and form a single vessel, the diameter of this vessel must be in proportion to the collected diameter of the whole ten thousand. This rule holds good in the vascular and in the glandular system."—P. 76.

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* Vide appendix, No. 21. This is a short extract from the description of Ferrein.

† Iterum iterumque ad corticem usque bifurcantur, eadem diametro, qua ductus serpentini. Muller de Fabrica Glandularum.

The urinary ducts, the author adds, are accompanied by minute twigs of arteries and veins in their course through the vascular or cortical substance of the kidney; but these vessels are greatly diminished in number when the ducts reach the medullary substance. The arteries with the *corpora globosa* lie on the coats of the ducts, and the veins accompany them. He further thinks that the *corpora globosa* or Malpighian bodies have no connection whatever with the urinary ducts, and perform no office in the secretion of urine.

Though we regard it our duty in the account of a performance like that of Mr Corfe, rather to record those facts which the author conceives he has ascertained, and to state those inferences and speculative views which he thinks himself entitled to form, than to express any opinion either on the correctness of the former or the merits of the latter, yet we cannot refrain from observing, that the opinion expressed above as to ducts being smaller than those from which they seem to proceed by bifurcation or division, appears to us not to be in any way tenable;—certainly not to follow as a necessary result of bifurcation. Mr Corfe appears to think, that excretory ducts are exactly in the same position as arteries and veins, in which, when one artery terminates in two or three, these branches are smaller than the trunk, and that when two or three veins conjoin to form one trunk, the latter is larger than the former. So far as the question appears intelligible, and the facts susceptible of comparison, excretory ducts are not in the circumstances in which arteries and veins are placed. Arteries and veins necessarily almost, at least anatomically, consist of different orders, decreasing and increasing. Excretory ducts, or secretory tubules rather, are all upon an equality as to size; at least there is no anatomical or physiological reason why they should not be equal in diameter and calibre. The fluid which flows through arteries is propelled by an impelling agent onwards from the large vessels to the small, and from the small to the smallest. That which is conveyed back to the heart by the veins is collected from innumerable points, and progressively carried into larger channels, from which again it is conveyed into the largest trunks. In this there is a reason, both anatomical and physiological, for the vessels gradually diminishing in size and increasing in number as they proceed from the centre to the circumference, and, conversely, increasing in size and diminishing in number as they advance from the circumference to the centre.

With secreting tubules, on the other hand, it is altogether different. There is no progression of the kind taking place in arteries and veins. When the materials of secretion are brought to the capillary vessels, it is impossible to perceive any necessity, either anatomical or physiological, either mechanical or vital, for the vessels through which the secreted product is ultimately to pass, being of gradually diminishing size and calibre. They may be all of the same diameter, so far as the purpose to which they are subservient, is concerned. Such at least, if we are allowed to apply mere reasoning to this subject, must be the mode in which we reason, and the conclusions at which we arrive.

Even, however, in the vascular system, we find evidence analogous at least, if not direct, to show that these secreting or glandular tubules may be all of the same size and diameter. It is known that the leading and distinctive character of capillary vessels, the minute and final divisions of the vascular system, is, that these vessels are all equal in size and calibre, as nearly as possible. No ramification, no progressive decrease or increase in the comparative size of different sets or orders of vessels, is there observed. Nothing but an extensive and infinitely anastomosing or interlacing net-work of vessels, all equal in size, is there perceived. If, therefore, we are to apply to the secretory tubules reasonings deduced from the vascular system, consistency requires that we take that portion of the vascular system which most nearly resembles the glandular tubules. The inference then must be, that there is no reason why these tubules should not all be of the same size, and every reason why the size and diameter should be throughout the same.

Of the vascular system, another particular division affords yet another argument of the same kind, and not less conclusive, against the inferences which Mr Corfe deduces. Are not the minute branches or vessels of the lymphatic system all, as nearly as possible, of the same diameter and calibre? Look at the lymphatics on the surface of the liver, or over the coats of the intestines; and all seem, at their first origin from their respective sources, of the same size and diameter. It is only after multiplied minute vessels have united that a trunk of any considerable size is formed.

Here, therefore, we have another proof that the argument which Mr Corfe deduces from what is called bifurcation, in favour of the smaller and diminishing size of tubes from the bifurcating tube, does not hold good. The lymphatic vessels may be as justly taken to communicate an idea of the disposition and relative magnitudes of these secreting tubules, so far as bifurcation is concerned, as any other part of the animal economy; and indeed they and the capillary system together contradict rather decidedly the inferences which Mr Corfe deduces.

We have taken the liberty of thus applying mere reasoning to determine a point, to which it is manifest the author applies reasoning alone. It is our misfortune, that we cannot perceive that there is any force in his argument, that these ducts cannot be bifurcated or divided without undergoing a diminution in diameter. The whole difficulty consists in the use of the term bifurcation and division. Take away the term bifurcation, and say that these secreting tubules are united with each other and are separated from each other, that two or three are united into one, or joined into one, and that one may be joined or open into two or three, and the whole difficulty vanishes; and with it comes down the argument of Mr Corfe.

It must now be observed that upon this argument regarding the alleged effect of bifurcation depends in a great degree one of the most important points in the hypothesis of Mr Corfe, regarding the existence of these oil-tubes. What other anatomists, as Muller, Huschke, and Henle, denominate BIFURCATIONS of the URINARY TUBES, Mr Corfe maintains are the points of adjunction or coincidence of an oil-tube with an urinary duct. It thus appears that one of the most important and characteristic points of the hypothesis of Mr Corfe depends on the interpretation which is placed on certain appearances, on which the different observers are not perfectly agreed. It is not our intention to offer any opinion on the merits of this interpretation. We believe that it is extremely difficult to do so; and we think it is perfectly competent for every one to interpret in his own manner, and to the best of his judgment, obscure and uncertain microscopical phenomena, without thereby incurring either the censure or the hostility of those who differ from him in the mode of interpretation. But it is nevertheless necessary to specify the facts as they stand, and to distinguish accurately between them and the speculative inferences which they are made to sustain.

Mr Corfe represents the veins of the kidney as arising by minute vessels which are arranged so as to form circular or oval spaces. It has been already mentioned that within these circular spaces are seen the obtuse commencements of the urinary ducts.

The arteries, he further states, having been ramified through the gland, then form those cirrhose or curling arteriolets, which wind round and enclose the *corpora globosa*, and finally expand themselves towards the surface, in the hair-like vessels of the reticulated membrane. These hair-like vessels again form the medium of communication between the arteriolets and the rootlets of the veins. From them arise minute vessels, which may be traced into the veins; and if the veins be well injected, these hair-like or capillary veins, the author states, are likewise injected. Hence results the red zone around the surface of a well-injected kidney; which, he repeats, are the capillary divisions of the artery, when they have reached the surface of the gland (p. 77).

The veins having arranged themselves in the manner now mentioned, pour out some of the constituents of urine, from the venous blood into the urinary ducts. Having performed this office, each vein of the circle, or single mesh of the net, falls into a straight vein, which runs towards the centre of the gland. When the veins arrive at the spot at which the feathery oil-tubes form an adjunction with the urinary ducts, each of the vessels lies between two feathery oil-tubes, and passes into the semicircular oil-tubes, and then is immersed in the delicate oil enclosed within these semicircular oil-tubes. The oil is represented as intimately adherent to the coat of the vein, and this coat is further said to be more delicate than it was when the vessel is ramifying through the vascular substance of the gland. This arrangement is said, according to the hypothesis, to be subservient to a very important purpose in the process of secretion. The oil having sent off to the feathery oil-tube its redundant and excrementitious ingredients, is purified. It then exudes through these veins into the mass of venous blood, which is descending towards the *vena cava*, and thus passes into the general circulation. This admixture of OIL WITH VENOUS blood, as the latter is proceeding to the heart, takes place, the author believes, wholly on the internal surface of the gland. By internal surface, the author adds, that he understands that surface which is nearest to the centre or pelvis, or opening of the ureter (p. 78).

This inference regarding the separation and purification of oil, he founds chiefly on the circumstance already mentioned, that the coat of the vein within the semicircular oil-tubes, is thin and delicate; whereas without these tubes it is firm and thick in texture; and because white oil is seen within the oil-tubes, it is never observed on the exterior of these bodies.

The seven semicircular oil-tubes usually enclose one vein each; so that these seven divisions of the vein pass out of the seven oil-tubes, and immediately unite themselves into two or more main trunks, which pass outwardly as far as the external margins of the gland, when they unite into one trunk, and form the renal vein, which at length enters the *vena cava*.

Mr Corfe, therefore, arrives in this manner at the conclusion that the urine is a secretion, not from arterial vessels, as was generally taught before Mr Bowman made known his account of the arteriolets and the circulation of the Malpighian bodies, but from a system of venous capillaries. It appears, therefore, that he and Mr

Bowman have separately arrived at the same inferences. Mr Corfe regards this as a principle in physiological science exactly similar to that which, he thinks, results from the facts communicated by Mr Kiernan regarding the circulation and secretion of the liver. The representations given by that anatomist regarding the distribution of the portal vein and the hepatic artery through the liver, and the circulation through the branches of these vessels, lead to the inference,—that bile is a secretion from the blood of the former vein, that the hepatic artery is exclusively a nutrient artery, and that the hepatic vein receives both arterial and portal blood. In like manner, the blood of the kidney, it appears, is converted into venous blood before it is conveyed into the proper secretory vessels; and in these the urine is secreting from the blood. But there is this additional and peculiar to the kidney, according to Mr Corfe, that the kidney is provided with a distinct set of vessels or tubes for the separation and purification of oil. The feathery oil-tubes pour out their drain or secretion; the veins or venous rootlets pour out their excrementitious matter; and the whole fluid thus prepared forms the secretion denominated urine. The renal artery, he maintains, is merely a nutrient vessel to the gland.

It is unnecessary here to recapitulate the points ascertained and the conclusions drawn by Mr Bowman regarding the curling arteriolets of the *corpora globosa*. They may be found given at large in the sixty-eighth volume (pp. 466, 467, and 468). The chief circumstance deserving attention is the fact, that Mr Corfe had arrived in 1839 at the same conclusion as to the disposition of these vessels, which was formed by Mr Bowman in 1842.

The *corpora globosa* have always been invested with difficulty and mystery; and it appears not very easy to understand why the arteriolets of these bodies should be made to wind and curl in the multiplied tortuosities which they present, unless it was to produce some effect on the blood which flows through their winding and tortuous channels. The above solution, which is given both by Mr Corfe and by Mr Bowman, answers as well as any other, and, in the absence of any one more plausible, may in the meantime be admitted.

This, however, is not the sole application which Mr Corfe makes of the *corpora globosa* and their arteriolets. If the reader looks back to volume sixty-eighth, p. 464, he will find it there stated that John Muller, in his examination of these bodies, had formed the opinion that the *vesiculæ* or membranous capsules of these bodies contain another matter besides these blood-vessels, and which other matter, he thought, adheres at one point only. The office of these bodies, which has so much perplexed other anatomists, appears to Mr Corfe simple and obvious. They are receptacles of oil for the nourishment of the kidney; and the

OTHER MATTER of Muller which they contain, he is assured, must be OIL, also destined for the nourishment of the whole gland (p. 80).

This oil, he tells his readers, is poured out from the delicate and convoluted vessels, the *vasa cirrhosa* of the Malpighian globules. These globules are in some instances bloody in the recent kidney. But this appearance, he adds, arises from extravasation of blood by over-fatigue or disease; as when the oil of the blood is diminished in quantity and altered in quality, exudation ensues exactly as it does in purpura, scurvy, and similar diseases.

In addition, however, to the oil already existing in the arterial blood as it arrives at the mouth of the kidney, fresh supplies are given by the artery as it proceeds through the duct to reach the sinus of the gland. Several white oil-tubes, says Mr Corfe, may be noticed issuing from the renal gland and oil; and these pierce the artery and are lost in its coats. Mr Corfe doubts not that these oil-tubes convey oil into the artery (p. 81).

Of the anatomical points which Mr Corfe conceives he has established, either from the researches of other inquirers or his own, the following short summary may give the reader a condensed view.

"1. When the renal artery is injected, the terminal ampullæ, or pellets (*corpora Malpighiana*) are injected.

"2. When the renal vein is injected, these pellets are rarely filled.

"3. When the artery is injected, the urinary ducts are never injected.

"4. When the vein is injected, the urinary ducts may possibly become injected also.

"5. The urinary ducts can be injected from the papillæ to the surface of the gland.

"6. When the vein, artery, and urinary ducts are all injected with different colours, there yet remains a considerable mass of white matter not injected, and not injectible.

"7. This non-injected and white matter consists of the innumerable white, semicircular, feathery, and lateral feathery oil-tubes, described in this work.

"8. We cannot inject the reflected membrane.

"9. We can inject the reticulated membrane.

"10. There are no veins nor arteries in the papillary portions of the cones injectible.

"11. There are many veins, and some arteries, in the tubular portion of the cones.

"12. The papillary portion of the cones is wholly made up of oil-tubes and urinary ducts.

"13. The tubular portion of the cones is composed of lateral feathery oil-tubes, urinary ducts, veins, and arteries.

"14. The vascular portion of the cones is composed of lateral feathery oil-tubes, urinary ducts, veins, and some arteries.

"15. The vascular or secreting portion of the gland is composed of innumerable feathery oil-tubes, urinary ducts, veins, arteries, lymphatics, and nerves, connected together by the reticulated membrane."—Pp. 183, 184.

To these may be subjoined, from the second work, the following aphoristical statements:—

"2. That these oil-tubes coalesce with, and drain their contents into, the urinary ducts.

"3. That the radicles of these oil-tubes are so perfectly indestructible in their nature, that, when the most complete decomposition and disorganization of the whole kidney has been obtained by long-continued maceration, yet these oil-tubes remain unchanged in their nature, like the fat which they contain.

"4. That these oil-tubes drain off a large portion of water from the oil which surrounds the kidney, and carry it into the urinary ducts for excretion.

"5. That these oil-tubes were the 'veins' alluded to by Carpi and Mathæus de Gradi; 'the second class of ducts, or glandular ducts,' of Bertin; the 'mèches' of Winslow; the 'vaisseaux spongieux' of Vieussens; the 'ducts from veins' of Eysenhardt; the 'white cortical ducts' of Ferrein; and the 'serpentine ducts' of Müller, Ruysch, Meckel, Rathke, Schumlansky, Huschke, &c., and the 'lymphatic vessels' of Nuck, &c.

"6. That these authors had no idea that the office of the kidney is twofold; for, whilst, on the one hand, it separates saline excrementitious matters from venous blood, it removes the watery portions, with the elements of urea, from the fat of the body, thus exhibiting a close analogy to the functions of the liver; for, as the hepatic artery is primarily a nutritive vessel, and, secondarily, aids in the formation of bile, and the portal vein is charged with highly oleaginous and non-coagulable blood, for the secretion of bile, so also, the renal artery is a nutrient vessel, whilst the venous plexuses adjacent, and oil-tubes combined, separate the constituent principles of healthy urine. So that I have observed, that wherever we find no portal blood going into the kidney, &c., we may trace the fat passing into this gland through its proper membranous sheath; oil-tubes pass off from these membraneous sheaths or calyces, and these tubes anastomose freely with the vascular network of veins which surround the whole course of the uriniferous ducts. In some reptiles, birds, and fishes, a portion of venous or portal blood, from the intestinal canal, the extremities, and the tail, not only goes to the grape-like fat cells of the liver, for the secretion of bile, but a portion of this venous or rudimentary portal system also goes to the kidney for the secretion of urine."—P. 87.

The author next compares the physiological relations of the kidney with those of the liver; and maintains that there exists between the circulation and mode of secretion of these two glands the closest similarity. One step to this result has already been taken in showing that urine is a secretion, not from arterial, but from venous blood. Another step consists in proving that to the secretions of both glands, both venous blood and oil are essential contributing ingredients.

The liver, he conceives, forms the pattern or model for all the glands. Since it has no fat within or around it, as is the case with other glands; the portal vein necessarily performs the office of conveying both oil and venous blood to the liver. He then adduces from various anatomical writers evidence to show that fat is actually conveyed by the afferent veins of the VENA PORTÆ to the liver. The omentum, he maintains, performs to the liver an office quite similar to that which the renal fat performs to the kidneys. It is certainly in favour of this view, that in various animals, especially FISHES, the liver does contain a large quantity of oil; and that even in the normal state oil is found in variable proportions in the liver of the Mammalia. Mr Corfe thinks that it will be found that the bile contains less adipocire or stearine, and more elaine, than renal oil. It is known that bile contains one fatty principle, namely cholesterine, or the elements of this principle.

As, however, it is necessary to the solidity and plausibility of the hypothesis now brought forward, that the blood be shown to contain oil, Mr Corfe next enters upon this department. Here we do not propose to follow him, as, while he is rather discursive and prone to repetition in general, he, in this part of his argument, wanders into a most extensive and devious course. It may be sufficient to say, that Dr Babington proved in 1830, that oil, in the normal state, is contained in the blood; and that this inference has been since that time confirmed by other inquirers, as Lecanu, Poggiale, Boussingault, and other analysts.

Albumen also, as the crust in the blood, he maintains, is nothing but a watery solution of oil, without any of that crystallizable base (stearine) which is found for the most part in animal fat (p. 127).

Indeed, if fatty matter be not taken directly into the stomachs of animals, it appears from well-established observations, that it is readily prepared in the alimentary canal by catalytic reduction of the food, and that, in this manner, considerable quantities of oil are incessantly conveyed into the sanguiferous system, in order to be applied as the exigencies of the economy require. This therefore must be conceded as a principle established.

Mr Corfe next treats of the circulating animal oils; and here he is scarcely less discursive. It is sufficient to say, that he regards the fat found in the adipose membrane, round the joints, round various organs, as the heart, the kidneys, the intestines, as all serving important purposes in the nutrition of the animal; that it is undergoing alternate deposition in its proper receptacles, and absorption from these into the circulation; that it is conveyed from the alimentary canal into the chyliferous system, and blood-vessels; that from these it is conveyed, and distributed into various regions, where it is made subservient to nutrition and secretion; and that the lymphatics are the great oil-ducts which carry oil into the blood. The lymphatics, however, in this matter, he distinguishes from the absorbents, as the latter do not enter glands, while the lymphatics or oil-ducts, he allows, do enter these organs (p. 182).

The following extract gives the leading ideas of the author on this distinction, between these two orders of vessels:—

" *Facts in physiology and in medicine, in proof of two distinct sets of vessels, oil-ducts, erroneously termed lymphatics, and the true absorbents, that is, the lacteals, and other glandular absorbents, and the non-glandular absorbents, or waste pipes.*

" 1. Inunction of mercury on the skin, especially if the surface be inflamed, produces dystenery, sloughing ulcerations of the large intestines, but not of the small ones.

" 2. Effused fluids, as in empyema, and peritonitis, &c., when removed by absorption, gives rise to a diarrhœa, hence termed a 'critical purging.' I have often inspected the evacuated matters in such instances, and have invariably found the substances not fæcal matter, but a fluid resembling the white of an egg, or albuminous shreds, with some tinge of bile.

" 3. If there be inflammation of the absorbents, the larger veins do not become implicated in that inflammation, but there is a circumscribed abscess of the limb, and thus the inflammatory action resolves itself into a termination.

" 4. If there be inflammation of the lymphatics, or oil-ducts of a limb, the veins are sooner or later the seat of a similar inflammation; hence phlebitis, and obstructed circulation, œdematous feet, &c., in the ankles of weak cachectic females, and in old people.

" 5. In scarlet fever, a disease which consists in the admission of red blood into the absorbents, the whole intestinal canal is equally as red and vascular as the surface of the skin. As the one dies away, so does the other fade.

" 6. Milk, urine, bile, &c., when once secreted from oil, and sent into their appropriate ducts, are never 'absorbed again by lymphatics, to be carried into the blood,' as Cruikshanks asserts.

" 7. The fluid in the lymphatic or 'oil'-glands, has globular particles in it, similar to those in milk. This fluid is found in the glands over all the body. It is greatest in quantity in young and growing animals, where more blood is required; but it is scarcely discoverable in old age. This is oil, I believe, passing out of the several reservoirs of oil, through glands, to go into the blood.

" 8. Dr Wm. Hunter, Messrs Hunter, Hewson, and Cruikshanks assert, that all lymphatics pass through glands, but deny two sets of vessels, lymphatics and absorbents; hence the latter author is perplexed by and cannot explain the following fact.

"'Mr Hewson had an idea that there were lymphatics which entered the thoracic duct, without having passed through any gland; and that the duct might be injected from the great toe, without any gland being filled in the whole course of the vessels. I have injected the thoracic duct from the lymphatics on the back without injecting any gland. I confess that I have injected lymphatic vessels from the great toe, which ran along the lower extremity, and passed under Poupart's ligament, without having previously entered any gland.' These injected vessels were lymphatics, or oil-ducts, not entering glands. It is not necessary they should do so always, and we find it constantly occurring in the animal frame. But they do enter glands usually; the reverse is the exception to the rule."—Pp. 185, 186.

We must now, however, advert shortly to the views which the author entertains on the morbid states of the kidney, and the morbid states of the system at large arising from disorders of the kidney.

Mr Corfe addresses himself first to the brick-dust sediment, and sandy deposit, which is the result of the presence of uric acid in the blood. He gives the usual pathological speculations, and the usual therapeutic directions, and on these we do not dwell.

He next speaks of the frothy state of the urine, which is indicative of a condition of system, if not more serious, at least often more unmanageable.

This frothy state is apt, he says, if neglected, to pass into another more marked change in the urinary secretion; and this consists in the formation of a greasy unctuous pellicle on the urine when cold, having also when the light falls on it a variegated colour, the leading tints being pale blue, red and green, or greenish yellow. This appearance Mr Corfe does not individualize. We believe it in general denotes the presence of the triple phosphate, or at least a state of urine proceeding to the phosphatic diathesis. He allows that it is seen in some disorders of the chest, but more especially in those persons who are disposed to consumption and diseases of a debilitating character. It further denotes imperfect digestion, and impairment of the energy of this function and of many other vital actions. It is in truth always associated with a debilitated condition of the frame.

The remedies recommended are the use of quinine, sarsaparilla, especially the liquid essence of the root, steel, and tepid saline baths.

The great evil, however, consists in the morbid and perverted states of the renal and general oil. The morbid states to which the kidney is liable are, according to Mr Corfe,—first, a superabundance of animal oil; secondly, a deficiency of animal oil; and, thirdly, a decomposition of animal oil.

An excess of oils in the cells of the adipose membrane may lay the foundation of a distinct form of dropsy; while a similar excess in the circulating fluids of the body, but more especially of the blood, may cause a form of rheumatism.

BRAWNY OR HARD DROPSY, as often observed in connection with disease of the kidney, he properly distinguishes from the œdematous or DOUGHY DROPSY. He allows, however, that the former may after the lapse of weeks pass into the doughy dropsy; in which case the disease usually proves fatal in a very short time.

The truth is, that the hard or brawny dropsy is most commonly an inflammatory disorder, taking place in an inflammatory state of the system, with sizy blood, tense hard pulse, and, not unusually, indications of inflammation of some internal organ, as the lungs, the liver, the kidney. If the inflammatory stage be passed over and neglected, without the employment of appropriate remedies, then it assumes the more serious and fatal character of soft, doughy, or cold dropsy (p. 233).

This disease, the author maintains, derives its characteristic features from a pathological change in the subcutaneoùs oil, which keeps up a reciprocal morbid action in the kidneys and their oils.

It is most commonly met with in stout men betwixt the ages of forty and sixty; and rarely before or after that period of life. Persons attacked by it have most usually a well-formed, strong, bony fabric of body, and are rather square-built plethoric men, averaging five feet eight or ten inches in height. The disease, he says, can rarely be traced to any previous organic changes in the body, as diseased heart, diseased liver, or diseased kidney; but it occurs suddenly, at a time when the individual thinks himself in excellent health. The usual causes of its induction are exposure to cold when overheated, exposure to cold during sleep, especially upon the ground; and consequently it often appears in persons who have been previously making some great and continuous exertion.

Whatever, however, be the main exciting cause, the mode of operation is in all instances the same. The skin is all at once thrown into a state of disorder; its secretion is suppressed; and its circulation is perverted and disturbed. The two drains from animal oil, the sweat and the urine, become deficient, altered, and deranged. The circulating glandular oil of the kidney passes into the blood in an impure and watery state. The saline matters of this vital fluid (the blood) are also disordered, causing febrile heat and disorder at the commencement of the attack.

Though the abdomen is swelled, no fluctuation is detected. The legs and thighs are swelled; but the puncture of a needle is followed by no escape of fluid. The reason is that the cellular tissue is loaded with a thick, impure, inspissated oil, the presence of which prevents serum from escaping.

Mr Corfe allows that if blood were drawn from the region of the kidneys in the early stage of the disease, we suppose before the limbs have become swelled, it would often remove the disorder, and prevent the patient from becoming dropsical. Doubtless it would; and, even when the swelling has taken place, a good large blood-letting from the arm is by far the most speedy and effectual method of cure.

Here we must be permitted to observe that the author is betrayed into a degree of inconsistency, as to the statement that there is no disease of any internal organ. It may be, that there is no organic change. But in every case of this kind the kidney is congested, its entire vascular system most thoroughly loaded, and its circulation obstructed. If the disease be neglected and overlooked, either the patient recovers very slowly, or it passes into one or other of the forms of granular disease of the kidney.

It is worthy of remark, that Mr Corfe slips insensibly from this acute form of the disease, in which he represents no organic disease to be present, to the most serious forms of organic change in the kidney. Here, however, he must be allowed to speak for himself.

> "It has been already stated, that the source of the attack might be invariably traced to a preponderance of oil in the cells of the adipose membrane, and in the log of renal oil. The individual, prior to his attack, finds himself growing stout and lusty; this increase of size depends upon increased deposits of fat in the various cells of the cellular membrane. The subcutaneous adipose tissue, and the larger cavities, as well as the heart, &c., become so many beds for fresh depositions of fat. The oil cells are, at length, so crowded by the innumerable and increased quantity of the globules of oil, that the healthy circulation of this fluid is retarded.
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> "During this sluggish state of the fluids, the blood does not receive its due supply of healthy and pure oil from the superficial and deep-seated oil-ducts, or lymphatics. The blood, thus clogged in its progress through the various minute tissues of the body, and unable to overcome the obstruction which surrounds its capillary vessels, exudes its serous or watery globules into these bloated oil cells. Whilst this effort is being made on the part of the blood in its vessels, the surface of the limbs presents an extraordinary degree of hardness. But when these changes have gone through their first stages, and the capillaries are in some measure relieved, the board-like hardness of the limbs diminishes, and the brawny character of the surface somewhat disappears.
>
> "But, during the progress of these changes in the oil and in the blood, there are others of a more serious nature going forward si-

multaneously in and around the kidneys. These wonderful organs, the main purifiers of animal oil, become inclosed and imbedded in their own fat; their tubular portions are encroached upon, and impacted by, shapeless masses of fat. This unnatural growth within and around parts so exquisitely delicate in structure, and so highly important in function, produce most serious disturbances in the action of the whole organ. Many instances have occurred where the tubular structure of the kidney was supplanted by huge lumps of this hypertrophied fat, and nothing remained around them but a small portion of the vascular substance of the organ. Such preparations I have now in my possession, where the brawny dropsy terminated fatally.

"The vascular substance, under such circumstances, is crimped, hardened, and very red, with a yellowish tinge or ground. It answers to the first of the three divisions of diseased kidney given by Dr Bright, in the following words:—'In the first a state of degeneracy exists, which, from its appearance, might be regarded as little more than simple debility of the organ (anæmia). In this case the kidney loses its usual firmness, becomes of a yellow mottled appearance, and, when a section is made, nearly the same yellow colour is seen to pervade the whole of the cortical part, and the tubular portions are of a brighter colour than natural. The size of the kidney is not materially altered, nor is there any obvious morbid deposit.' In the most advanced state of the disease, Dr Bright thinks there is a more decided alteration of structure, and that some portions are consolidated, so as to admit of very partial circulation, in which case the surface has a somewhat tuberculated appearance; the projections being paler than the rest, and at this period the urine is coagulable. I cannot say that I have found the last statement respecting the urine to be an invariable fact, yet I can readily believe that it is as the doctor asserts. Had this eminent physician been aware of the important office of the animal oil within the kidney, and of the anatomical structure and physiological uses of the feathery oil-tubes of this organ, his valuable researches upon the diseases of this gland would have been proportionably intresting.

"When this diseased state of the kidney supervenes, the blood becomes poisoned from the various salts, which this organ should drain away, being retained in it. Urea then circulates, headach, and disturbed mental faculties creep on. Palpitation and emphysema, with its consequent dyspnœa and hydrothorax, shortly set in, and thus the case becomes more and more aggravated, and is less under the control of remedial agents, and at length the patient sinks from some internal effusion.

"But I should have remarked that the liver becomes, in the majority of cases, the seat of fatty accumulations also; hence a greasy or caseous liver is by no means an uncommon pathological feature in this fatal disease. The heart, from the impeded circulation of the liver, becomes thin in its walls, and dilated in its cavities,

fat accumulates around its base and apex, and small fatty pendulous bodies are occasionally met with on its semilunar and mitral valves.

"With respect to the treatment of this form of dropsy, it may be observed, that in most instances, as far as my observation goes, an uniform antiphlogistic plan will answer best. When the medical man has the charge of his patient in the early stages of the disease, he should bleed him from the arm somewhat copiously; and as soon as the system rallies in some measure from it, he should follow up the depletion by cupping him to ten or twelve ounces from the region of the kidneys, and administer some brisk saline purgatives, keep him on low diet, and allow him to drink freely of lemonade with cream of tartar, ordinarily termed 'imperial.' The akaline carbonates, with digitalis and blue pill, are very advantageous in this stage. He should allow his patient a warm bath two or three times a week. Or, if this cannot be procured, let him have an oblong cradle-like wicker basket made, with a board at the feet, and open at the other end for the head; a hole made in this board to admit an iron tube will complete the apparatus; to the other end of the tube a spirit lamp should be placed; and thus, as he lies in bed, a hot-air bath can be administered to him. This is a very valuable remedy when duly applied, and will oftentimes excite a free action of the skin when diaphoretics have failed, so that the kidneys have been relieved of their congestion, and the dropsy has gradually disappeared. If the disease should not yield under the above treatment, ten to twenty minims of turpentine, in the form of an emulsion, has frequently succeeded in arresting its further progress. Slight friction over the kidneys, and hip baths, are also highly serviceable."—Pp. 237–239.

The following observations, on the influence of this condition in producing cutaneous eruptions, deserve attention:—

"Before I conclude this head of diseased oils, there are other minor alterations in this vital fluid to be noticed, which give rise to painful and troublesome complaints, which are frequently far more intractable than those which involve the life of the patient. The disorders alluded to are certain diseases of the skin, which originate in a superabundance and rancidity of oil in the subcutaneous cellular tissue; amongst these may be mentioned that troublesome malady, in strong and vigorous children, called porrigo. The eruption originates from disorder in the oil and oil cells of the skin. This animal substance becomes corrosive or pungent in its character, and gives rise to most offensive acrid discharges. It is constantly met with among the children of the lower classes, where cleanliness of person is not properly attended to. When a hair is pulled out, the root or bulb is found diminished in bulk, and the extremity of it nipped off, as though it had become ulcerated and destroyed. This arises from the deficient nourishment which it receives from the oil-cell, into which each bulb passes; or it may be caused by the acrid nature of the oil, which ulcerates, and wastes the delicate root."—Pp. 239, 240.

Rheumatic fever, or acute rheumatism, in the same manner, the author ascribes to perversion in the animal oils. The joints, he thinks, are formed like so many little or large circles in the body, around and within which these oil-ducts and capillary vessels freely inosculate; and similar anastomoses, to the same extent, and in the same manner, cannot be traced in any part of the body. (P. 242).

Rheumatism, he distinguishes into the acute form, and that which he calls serous rheumatism. In the latter, he observes, which is a milder form of the disease, the joint, though surrounded by a bag of fluid, is flexible to a great extent without inconvenience to the patient. The reason of this difference is, he states, that, in the acute disease, the deep and superficial oil-ducts, namely, those lying upon the surface of the synovial membrane, and which afford the lubricating joint-oil, as well as those that communicate freely with them, and are scattered throughout the ligamentous bands of the joint, are equally inflamed from the acrid nature of the oil which they contain. It is otherwise with the serous form of the disease. In this, the oil-ducts of the bursæ of the joints and their tendons only are implicated; the cavity of the joint is free from disturbance, and hence the articular ends of the bones may be moved over each other with less inconvenience.

He afterwards applies the same doctrine in order to explain the occurrence of *pericarditis* in the course of rheumatic disorders.

Next comes chronic rheumatism, the stiffness and immobility of the joints in which, are upon the same principle ascribed to the presence of solidified oil within and around the joints, and perverted and morbid states of the renal and the circulating oil.

Mr Corfe then recurs to the subject of granular disease of the kidney.

"If the circulating glandular oil, that enters the interior of the kidney for purification, or to be drained, in other words, becomes inspissated, from whatsoever cause, it will necessarily follow that the minute extremities of these tubes in the vascular portion of the kidney, become blocked up by the concretion thus formed. Extreme turgescence of the whole organ immediately ensues, the blood vessels are loaded, the Malpighian bodies lose their natural paleness, and are, more or less, sanguineous in colour. The tubular portions assume a chocolate tinge, the whole gland swell, and is flabby in consistence; the oil is of a dark ochrey colour, and the blood remains universally fluid after death, giving a red tinge to its vessels. This rapid and fatal change constitutes the first stage of the disease, so ably described by Dr Bright. The patient is oftentimes carried off by a sudden attack of convulsions, and there is bloody urine, delirium, urinous breath, coma, epilepsy, and speedy dissolution. In these instances the blood is as much poisoned by the deleterious fluid that passes into it, as well as by what is retained in its vessels, as is the blood of the Indian who dies from the bite of a venomous reptile. The very vitals are at once struck at, and the life-giving property of the blood is destroyed, and consequently, its power of coagulating ceases with a cessation of its life. The kidneys, under such circumstances, present a deep modena red in colour, and are highly injectible."—Pp. 264, 265.

"The expression, granular degeneration of this organ, gives an imperfect notion of this disease; for it often happens that the whole series of oil-tubes is more or less obliterated by concretions of oil within them, and yet is the organ smooth, and without the least appearance of a granulated structure. The rough or scabrous surface of a granular kidney is an advanced state of disease, and is only seen when the Malpighian bodies are involved in the general derangement of this organ. These little cryptæ then become hypertrophied, indurated, and white, giving to the organ the appearance of fine sand, or of mustard seeds sprinkled over it. The terminal arteries of the kidney are never capable of being injected when the disease has advanced thus far in progress. But the first change in such kidneys is a total or partial impermeability of the oil-tubes from inspissated oil just as they adjoin the urinary ducts, the consequence of which is, that these latter portions become inflamed on their surfaces, from an absence of the lubricating fluid drain from oil. Blood as well as urine flows from them. The surface of the kidney, where these ducts take their origin, is mottled with dark red or black spots, the result of extravasation of venous blood from the vessels of the reticulated membrane around the venous circles of the gland. This extravasation may be so severe, in the early stages of the disease, that congestion of the blood-vessels within the Malpighian bodies ensues, and these rounded pellets are also found with a sanguineous purple tint."—P. 274.

It may be allowed that these descriptions communicate just notions of the state of the kidney in the several forms of this disorder; and it is creditable both to the diligence and the acuteness of Mr Corfe that the general correctness of these descriptions has been confirmed by various subsequent inquirers. It is unnecessary to repeat the descriptions given by Dr Johnson and Mr Toynbee, because these are already submitted in the sixty-eighth volume. But it may be satisfactory to subjoin the following account, as condensed from Gluge, who distinguishes three forms, or probably stages, of the degeneration.

Gluge maintains, from microscopical examination of kidneys in this state, that the infiltrated matter is in general oil or fat.

In the first form, which is that in which the surface of the kidney is yellowish with red points and millet seed-like granulations, inflammation, with inflammatory globules and purulent matter, is associated with fatty infiltration or *steatosis*. The *tubuli*, especially those of the cortical substance, are filled with fat globules large and small. These distend the tubuli, the convolutions of which form the granulations, and afterwards appear more in the medullary (straight tubular) substance. At length the fat globules are effused between the *tubuli*, and form masses of 10, 15, or more fat-globules, which are occasionally enclosed within a membrane. The Malpighian bodies and the capillary vessels are at first filled with blood; but in the further progress of the disease they are bloodless and pale. The vessels of the medullary substance undergo a like change, though to a smaller degree. Not unfrequently the membrane of the *tubuli* entirely disappears. The fat globules observe in their mode of deposition at first the direction of these *tubuli*; afterwards this is lost, and a confused mass of fat-globules takes their place. In rare instances the fat-globules are deposited not at first in the tubuli, but in the blood-vessels and Malpighian bodies.

In the second form, in which the kidneys are red-brown in their cortical as well as in their medullary substance, and in which, in consequence of a reddish striated arrangement of the medullary, the two portions can scarcely be distinguished;—the renal substance is recognized as a reddish, soft, almost soluble, jelly-like matter, in which only the cellular frame-work with wide meshes appears as a separate solid element; no trace of tubules or blood-vessels is to be found either in the cortical or medullary matter; and fat-globules, scattered among tubules and empty colourless Malpighian bodies, lie in the residual cellular texture of the glands.

In the third form, in which the outline of the kidneys remains unchanged or diminished, and their outer surface and the section of the cortical substance is occupied with prominent pisiform granulations, giving the glands a rough aspect and hardish appearance; sometimes inflammatory globules are found with fat-globules; yet these are always in smaller proportion; the granulations contain many urinary tubules dilated by a yellowish granular mass and fat-globules, which are also deposited between the tubules.

In short, the fat globules may be deposited within the tubules, without them, and between them, and without or around the Malpighian bodies.*

According to Vogel, the cortical substance in one form of the disease, which is the inflammatory, is thick, white-yellowish, variegated with red dots and lines, and very compact like lard. The substance contains little blood. The vessels of the Malpighian bodies are much less distinct than in the normal state. The tubules are indistinct and confused, and between and around them plastic matter is infiltrated. A similar infiltration is observed among the medullary tubules.*

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* Atlas der Pathologischen Anatomie. Stearose der Niere, Taf. 3.

In the foregoing abstract of the contents of the volume of Mr Corfe, we have given, in as condensed a shape as possible, only what relates to the anatomy, physiology, and diseases of the kidney. We have avoided in general expressing any opinion on the merits of the alleged discoveries and speculations of Mr Corfe, because we think it is best to allow readers to form their own judgment; and we think that this judgment the account now submitted will enable them to make. If it do not, it will form a reason for their perusing the work itself.

We have also avoided carefully referring to other matters which the author has rather freely and repeatedly introduced. Mr Corfe appears to be a man of deep religious impressions and strong devotional feelings; and to these he has given vent in the present volume rather more frequently than prudence and good taste would recommend. This it is not our intention to censure. But we may be permitted to remark, that the introduction of effusions such as are found in this work is calculated to produce several most hurtful consequences. Mr Corfe we believe to be as sincere as he is devout. But the display of these devotional and theological meditations is calculated rather to discredit true and genuine religion; and cannot fail to make the friends of pure Christianity wish, that another vehicle had been employed for conveying them to the public.

Not only, however, does the introduction of these effusions appear to be injudicious in the manner now mentioned. It is to us not doubtful that they have tended to depreciate the value of the really useful parts of the author's performance. Many persons, who observe the desultory manner in which the author proceeds, and find the frequently-recurring effusions of a theological and devotional character, will be at once deterred by this circumstance alone, from reposing in the scientific statements and anatomical descriptions that confidence to which their true merits give them a just title. It is with great respect for Mr Corfe as a person of evident diligence and acuteness that we feel compelled to say, that the mixing of religion and science in the manner employed in this work does injury to both. If Mr Corfe wishes to write a religious and devotional work, there is no reason in the world why he should not write it; and we sincerely believe, that it would find readers, and might be useful. Scientific subjects and scientific descriptions, however, are there out of place, and will accordingly be received as of doubtful merit and questionable authority. On the other hand, had Mr Corfe confined his labours solely to the making known of his own anatomical researches and his own physiological and pathological deductions, the present performance might have been condensed to one-third of its present size. Secondly, it would have attracted more notice, would have been received with greater confidence, and in all probability would have been more generally read. And, thirdly, the author might have avoided much repetition and desultory discussion, which renders it not an easy task to collect the really valuable matter.

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* Julii Vogel Icones Pathologicæ, Tabula xxvi. p. 107.

With all these drawbacks, nevertheless, the work is one of great merit and originality.

ART. II.—*Physiological, Anatomical, and Pathological Researches.* By JOHN REID, M. D., Fellow of the Royal College of Physicians, Edinburgh, Chandos Professor of Anatomy and Medicine in the University of St Andrew's, &c. &c. Edinburgh and London, 1848. 8vo, pp. 659. Five Plates.

OF the twenty-eight important memoirs contained in the present work, eleven appeared in successive volumes of this Journal between the years 1835* and 1843.† These are, the 2d, 3d, 4th, 5th, 6th, 7th, 9th, 14th, 15th, 19th, and 21st, with part of the 22d article. It would be difficult to pronounce on these memoirs a higher eulogium than is implied in this proceeding.

Of the other seventeen articles, eight appeared in the pages of two contemporary journals. These are, the 1st, 12th, 13th, 16th, 17th, 18th, 20th, and part of the 22d; and the subjects of which they treat are the following.

1. On the relation between muscular contractility and the nervous system; 12th, on the measurements of the heart; 13th, tables of weight of some of the most important organs of the body at different periods of life; 16th, three cases of partial hypertrophy of a portion of the organs of voluntary motion; 17th, case of monstrosity by inclusion; and a case in which both kidneys were placed on the same side of the body; 18th, case of disease of the spinal chord from exostosis of the second cervical vertebra; 20th, observations on the statistics and pathology of the continued fever of Edinburgh; 22d, contributions to forensic medicine, part of which appeared in this Journal.

The 10th essay is part of a lecture on sensational and emotional reflex action. The 11th, on some parts in the anatomy and physiology of the heart, is from the article on that organ in the Cyclopædia of Anatomy and Physiology.

The 23d article contains a short summary of remarks on the value to be attached to experiments on the nervous system, derived from a lecture delivered some years previously.

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* Edinburgh Medical and Surgical Journal, vol. xliii., 1835.

† Ibid., vol. lix. Edinburgh, 1843.

The information communicated on these subjects bears the impression of the diligence, deliberation, and accuracy by which all the labours of the late Dr John Reid are distinguished.

It is proper to say, that to such of the papers as required additions, the author has subjoined, in the form of appendix, whatever additional information subsequent research has supplied; and he has, in this manner, without altering the original cast of his own papers, placed before the reader complete views of each subject at the date of the publication of the present collection.

The 24th article is a review of the treatise of Professor C. Joseph Edler Von Wattmann on the speedily fatal effects of the admission of air into the veins. This the author states that he has been induced to reprint, as "he believes that he has there given a fair and complete account of our knowledge on this subject, up to the time it was written, September 1844." The subject was examined at length, historically, critically, and physiologically, in the fifty-fourth volume of this Journal, in 1840, when the Treatise of M. Amussat was considered;* and before or subsequent to that time, the pages of this Journal recorded various instances of the accident from different sources, and became the channel of publication for the elaborate paper of Mr Erichsen.† The author has gone over the same ground so closely that it becomes unnecessary at present to recur to the subject; and it only remains to say that the paper of Dr Reid presents a careful summary of information.

The 25th article is in like manner a review. The subject is the Treatise of Dr George Burrows on the Disorders of the Cerebral Circulation, and on the Connection between Affections of the Brain and Diseases of the Heart. The article is critical and something controversial. It is not exactly in our way; and it is enough to mention the fact, without expressing any opinion on its merits.

The 25th article, consisting of Anatomical and Physiological observations on some Zoophytes; the 27th, on the development of the ova of the nudibranchiate MOLLUSCA; and the 28th, entitled Observations on the Development of the Medusae,—were all previously published in different numbers of the Annals and Magazine of Natural History, between December 1845 and January 1848.

These memoirs contain much interesting information on the habits and functions of the animals of which the author treats.

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* Edinburgh Medical and Surgical Journal, vol. liv., p. 176.

† Ibid., vol. lxi., January 1844, p. 1.

The facts appear to have been carefully observed, and are distinctly narrated.

It will undoubtedly be allowed by all competent judges, that in republishing in the collected shape his several essays on anatomy, physiology, pathology, and natural history, Dr John Reid has not only performed an act of justice to his own reputation, and it may now be said, to his memory, but he has rendered a great service to those readers who take interest in the progress of anatomical and physiological inquiry.

Art. III.—*On Gout; its History, its Causes, and its Cure.* By William Gairdner, M.D. London, 1849. Post 8vo, pp. 232.

In undertaking to elucidate the nature of gout, and simplify and improve the therapeutic principles on which it should be treated, Dr Gairdner impresses on the mind of his reader the circumstance of the extremely variable disposition of the disease. He remarks its readiness to show itself suddenly and without preliminary warning; its insidious and unobserved approaches on other occasions; and its being liable in both instances to be mistaken for other complaints, and to deceive even good observers. With the view of enabling his younger and less experienced brethren to avoid mistakes of this kind, Dr Gairdner gives in the first chapter of the present treatise a short view of the preliminary phenomena by which the disease is usually introduced, and in his second chapter a correct and circumstantial history of the symptoms by which the actual presence of the disease is denoted. He then describes its effects after repeated attacks have taken place; and explains the most frequent terminations of the disease, for instance, in general feebleness of the whole frame; in disease of the heart, disease of the brain, hemorrhages from the alimentary canal, and disease of the liver and kidneys.

The occurrence also of sudden and unlooked-for death at ages comparatively early, does not escape the notice of the author, who gives, from the practice of M. Broussais at the Val de Grace, an instructive example of death, by swooning, in the person of a soldier of 48, in whom evacuations, by means of leeches, had been carried to a considerable extent. Of this case, Dr Gairdner remarks that it was not surprizing, certainly, that the man died; yet it would not be true to say that he died of inanition. Many other patients in the same hospital suffered greater extremities of hunger than he did; nor did he show any unusual weakness. Dr Gairdner saw this man within an hour or two of his death, and found him cheerful, with a clear sonorous voice, hinting his dislike of doctors and their methods, and expressing his belief that half an hour of the wine-shop would restore him in a greater degree than all the physicians. Dr Gairdner concurs in the sentiment that this man was right in his own case. After, however, being duly depleted and starved, one day when he was talking cheerfully to his companion in the neighbouring bed, he felt a sensation of faintness suddenly come over him; he laid his head on the pillow, and expired.

After a most careful inspection of the body, no cause adequate to account for death could be recognised. The left ventricle was thought to be enlarged, and its walls attenuated.

The physician, in his clinical lecture on the case, though he did not see that his practice had any share in the event, perceived the great cause of the result to be gout, and gave the pupils various illustrations regarding the influence of the disease in perverting and masking the course of other maladies.

Cases such as this,—and they are not very unfrequent,—suggest the not very pleasant idea, that it had been better for the patient at least, if not more satisfactory for the physician, that the latter had discovered during the life of the patient, that gout was performing the principal part in the obscuration and disguisement of the real symptoms of the man's case.

From the facts now stated, Dr Gairdner draws the inference that instances of sudden dissolution, taking place in circumstances of this kind, are entirely owing to the influence exercised by gout on the brain and other portions of the nervous system, and through them on the heart; in other words, that the cause of death is dynamic not organic, and is of the nature of a fit of faintness or syncope. If this explanation removes not the difficulty of the case, it must at least be allowed to convey a useful lesson as a warning to practitioners, showing them that depletion and low diet, though often required by one class of symptoms, may in certain circumstances be carried to an injurious extreme. If, indeed, there be any truth in the hypothesis, that gout depends on the blood containing a noxious matter which ought to be expelled, and to which hypothesis Dr Gairdner shows a considerable degree of favour, then it may become matter of doubt and serious consideration how far depletion can be expected either to be useful or to be even not more or less injurious. If, as is now very generally believed, it be a well-founded doctrine, that gout depends on the presence of uric acid in the blood operating as a species of poison, and causing not only considerable disturbance in order to effect its elimination, but also exerting a direct noxious influence on many of the textures, then must it follow, that any amount of depletion beyond what may be sufficient to facilitate the efforts at elimination, and assuage the directly-irritating effects of the poison on the textures, must be detrimental. Circumstances may occur, even according to this hypothesis, in which it may be necessary not only to give up the system of depletion, but to allow the use of those articles of food and moderate nutriment, which may enable the system to contend successfully against the inherent enemy. Certain it is, that the therapeutic management of gout is in this respect peculiar, that though often induced and always aggravated by full living and excess, yet it does not well endure extreme depletion; and moderate nutriment, even with the judicious use of stimuli, is sometimes the channel by which the patient is restored to and continues in health. To this point, however, we may afterwards advert in its proper place.

Dr Gairdner, however, does not rest satisfied with leaving all cases of unexpected and speedy dissolution to be referred to the head of death by syncope, or by the influence of the nervous system over the heart being enfeebled and suspended. He properly observes, that in many instances the condition of this organ itself will best explain the event. In illustration of the justice of this opinion he gives, from the practice of his brother, Dr John Gairdner of Edinburgh, the particulars of an instance of fatal termination in an arthritic patient, in which the fatal event was distinctly traced to the circumstance, that the muscular fibres of the heart had undergone the adipose degeneration, and a similar change had taken place in the tubules of the kidney. The liver appears to have also been in the adipescent state. In this instance, in which the patient expired suddenly in bed, his medical attendant, after frequent examination of the action of the heart, could never detect any abnormal sound.

This condition of fatty degeneration of the heart, the author imagines to be the source of subsequent dilatation of the chambers of the organ. This may be the case. But much more commonly it is observed that the fatty degeneration merely enfeebles the heart to such a degree that the organ becomes unable to contract on and propel its contents, and consequently, either under the operation of some unusual cause, ceases to act, or in the case of considerable effort undergoes laceration.

Dr Gairdner allows, nevertheless, that the disease does not in all, he might have said not perhaps the majority of, instances assail in this manner the structure and action of the heart. The well-known fact, mentioned by Heberden and others, and stated also by the author, that many persons struggle with frequent attacks of the disease, yet after the lapse of some years seem to enjoy immunity from them, when all the vital powers are feeling the influence of the progress of decay, may be allowed to prove that gout either wears itself out, to use a common phrase, or that the cause of the distemper ceases to be produced within the system. Many deaths also, which have their first cause in gout, take place under such circumstances, that they are attributed to other diseases. Dr Gairdner is of opinion that it is impossible to believe the statement in the tables of the Registrar-General,—which gives 200 deaths annually in a mortality of about 350,000,—to be a correct representation of the mortality occasioned by gout. That it is not so, the conspicuous position assigned to this disease by insurance companies, he argues, distinctly shows.

We do not know, that this statement is to be regarded in the light of an imputation on the accuracy of the tables published by the Registrar-General; and even if it were, we believe that it might be left to that gentleman and his able coadjutor, Mr Farr, to defend and maintain the correctness of their numerical returns. But we doubt whether the author in this instance takes the view which is most likely to lead to correct results; and on this account it may not be unreasonable to subjoin only two remarks.

Gout is a disease which prevails chiefly in certain ranks and classes of society. It is unknown almost among the labouring and corporeally industrious classes. It is more common, on the other hand, among the wealthy, the luxurious, and those whose minds reqiure to be active while their persons are in a state of comparative indolence. It hence prevails most among those classes of society who have recourse to life insurance; and, accordingly, insurance offices attach great importance to its appearances as indications of the state of health and degree of insurability of the lives of applicants. Lawyers, clergymen (especially when in good rich livings), official persons with large salaries, wealthy country gentlemen,—these form the usual orders among whom gout displays its predilections.

Among the individuals composing the various ranks thus enumerated, however, gout is by no means, as gout, a common cause of death. Dissolution in such persons is much more frequently the consequence of apoplexy or palsy, or some disease of the brain, or its membranes; of disease of the heart and lungs, especially chronic *bronchitis* and the various affections ranged under the comprehensive abstraction of asthma; in some instances death may seem to be the effect of what is called hydrothorax; or it may be the result of disease of the arteries, of disease of the liver, or of disease of the kidney. Hence, in estimating the numerical proportions of deaths in this class, it becomes requisite to look not only under the head of gout, but under the head of the different diseases now specified. The individuals cut off under any one of the maladies now referred to may have been arthritic at one or several periods of their lives; or gout may have been latent in their systems. In short, though gout be not in its open character the cause of death, yet, under the shape of one or other of the maladies above enumerated, it may have undermined the the constitution and so contributed to the fatal event.

It may be further remarked, that the ideas of the actuaries of insurance offices on the predominent causes of mortality are not always correctly founded. They are suited for commercial purposes, and, as has been already suggested, they are accommodated to the classes of society who principally avail themselves of the advantages of life assurance. Rheumatism is probably a more fatal disease than gout; at all events, it often produces more decided and rapid mischief on the heart and its apparatus. Yet the very name of gout is supposed to imply, that a life is not insurable, or requires a very considerable additional premium; while rheumatic persons have often been received with little difficulty.

Dr Gairdner gives an instructive view of the characters of atonic gout and its most frequent metastatic forms. The metastasis affecting the heart he regards as not so frequent as that affecting the brain, which, in his experience, is more common than metastasis to any other region or organ. The forms of apoplexy and palsy, nevertheless, he finds to be the rarest shapes which the disease assumes. More commonly he has seen a kind of stupor, in which the patient preserves his senses of hearing and sight, but loses consciousness of persons and circumstances, place and time. He knows no one about him, not even his own family; his utterance is imperfect, or altogether gone; he seems like a person entranced; his eyes are vacant and staring; he understands some things said to him, and may do as he is bid, if what is required may be easily and quietly done. The pulse is full and hard.

This state is a species of what the ancient physicians called CATAPHORA, and is most frequently associated with a morbid state of the cerebral arteries; and though it occasionally goes off, yet it is liable to recur, and even without recurring, it may form the preliminary part of softening of some part of the brain. In some instances in which it goes off, and the patient wholly or nearly recovers, there is reason to believe that it depends on the poisonous operation either of uric acid or of urea and uric acid on the brain.

The hypothesis that the symptoms and effects of gout depend on the presence of uric acid, uneliminated, and acting consequently as an irritant poison, Dr Gairdner examines with care and judiciously. This hypothesis was originally devised and proposed in 1787 and 1793, fifty-seven years ago, by Murray Forbes, an intelligent surgeon in London, who, remarking the close connection between gout and gravel, and the tendency of both diseases to form concretions, conceived that gout depended on the presence of uric acid in the blood, which was liable to periodical deposition, through the medium of inflammation in organs, the vessels of which are of the smallest order, and whose disposition is inert; for example, tendons and ligaments; and the material cause of the deposition he considered to be the power and influence of some stronger acid either taken into the stomach orformed during the process of digestion.*

It is very remarkable how many proofs of probable correctness this hypothesis has, since it was first proposed, received by the successive labours of Wollaston, Prout, Dr Golding Bird, and Dr Garrod. Wollaston, by showing that the concretions called gout-stones consist of uric acid and soda, proved that uric acid must have been present in the blood, at the time at which these deposits were formed. Prout, by his correct and instructive views of the causes and effects of the uric acid diathesis and the lactic acid diathesis, traced the primordial movements of the disorder to errors in assimilation, in the course of which uric acid was either formed from disintegration of the tissues, or in some manner formed in the circulating fluid. And Dr Garrod, whose researches may be found in another part of this Number, discovering uric acid actually in the blood, must be allowed to have added an important link to the chain of evidence.

Admitting the full force of the facts ascertained by Dr Garrod, Dr Gairdner justly observes, that in order to establish the inference that the presence of uric acid in the blood is the cause of gout, it would be necessary to prove two things; first, that it always produces gout, and secondly, that in gout it is never absent. "I have seen," says Dr Gairdner, "and I am sure Dr Prout has seen, very durable deposits of urate of ammonia, in cases in which existed no great constitutional disturbance, for instance, in lepra and psoriasis, where no unusual disintegration of the tissues was going forward, and where no interruption to ordinary wholesome nourishment was suspected" (p. 69). This is in allusion to Dr Prout's doctrine of the disintegration of the gelatinous tissues being occasionally the source of the uric acid in the blood. "There is nothing," says Dr Gairdner, "of which I am more perfectly satisfied, than that I have often seen cases of true regular gout in which there was no evidence of excess of urates in the urine; and the cases are not rare, in which deposits of earthy phosphates, and mixture of the earthy phosphates with urates, are met with. This, indeed, might be expected in a disease occasioning so much of oppression of the brain, and so much disturbance of the nervous system."

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* A Treatise upon Gravel and Gout, in which their sources and connection are ascertained, &c. By Murray Forbes, member of the Surgeons' Company. London, 1793.

This is an important observation, and shows that Dr Gairdner has accurately observed the disease for himself. It perfectly accords with observations which it has occurred repeatedly to ourselves to make. Whatever may be the reason, certain it is that cases of gout often occur, in which the urine not only shows no indication of the uric acid diathesis beyond what is found in health, but cases in which it evinces decidedly either the presence of the phosphates, or that it is feebly acid, and is to become rapidly alkalescent. This also we have seen take place both in gout and in that form called rheumatic gout. The explanation, we confess, we do not understand. But the fact is not the less deserving of notice.

In 1786, nearly at the same time at which Murray Forbes solicited attention to the uric acid hypothesis, Berthollet had advanced the opinion that gout depends in some manner on the presence of an excess of phosphoric acid in the blood. From numerous observations, this able chemist inferred, that phosphoric acid is more sparing in the urine of the gouty, and in that of rheumatic persons, than in that of the healthy, and that during a paroxysm of gout it becomes more abundant, and equals the proportion found in the urine of most healthy individuals; and he therefore ascribed the phenomena of gout to the retention and accumulation of this acid, and its diffusion through the system.* Richerand, on the other hand, maintained, that, on the approach of gouty fits, the phosphoric ingredients of the urine are diminished, and seem to be conveyed to the joints, producing there arthritic effusions and concretions. It is probable that though these two opinions appear at first sight to be at variance, yet they may be reconciled, and shown to refer to the same process. Scudamore, indeed, confirms the results obtained by Berthollet, regarding the increase of the proportion of phosphoric acid during the gouty paroxysm; but he shows also that this acid is present in quantity in the urine in cases of inflammation of the skin, of acute rheumatism, of catarrh, of hepatic inflammation, of mesenteric tabes, of *mollities ossium*, and even in persons in the apparent enjoyment of good health.

It seems from all these facts and considerations probable, that the derangement both in the blood and in the urine in gout is not simple and always the same, but is liable to take different directions and produce different effects in different individuals.

Dr Gairdner is inclined to agree with Dr Garrod in thinking that uric acid is a necessary and essential ingredient of pure and healthy blood. This may be the fact; and it may be that minute quantities of uric acid may be present in a state of health without causing any conspicuous morbid phenomena. But it should be remembered that the experiments of Dr Garrod scarcely establish this inference in a decided manner. They merely show that uric acid may be found in the blood in persons enjoying apparent health, but in whom the excretory functions are not perfectly performed. No uric acid, it must be remembered, is formed in the blood of the sheep, nor any in that of fowls, though their urine consists of almost pure urate of ammonia. The first circumstance, we conceive, shows that in animals which live on articles containing little nitrogen, this excrementitial principle is not fromed in such abundance as to be left in the blood. From the second it may be inferred, that the energetic action of respiration by completely oxydating all the principles contained in or conveyed into the blood, prevents from being left in that fluid any uncombined nitrogenous principle, and ensures its elimination from the system in its proper form. It seems manifest that urea and uric acid are not so much formations as the refuse or rejected principles of those nitrogenous elements which are employed in nourishing and sustaining the albuminous and gelatinous tissues. Albumen and gelatine are taken from the blood and applied to the constant repair of the muscles, the tendons and ligaments, the fasciæ, the skin, and other gelatinous tissues. In the application of these materials, certain proportions of carbon, hydrogen, and azote are combined with certain proportions of oxygen. In these combinations, after the carbonaceous matter has been either applied to nutrition, or carried out of the system in the form of carbonic acid, and the nitrogen has been duly combined in the tissues in which its presence is required, certain proportions of nitrogen, oxygen, carbon, and hydrogen are left unappropriated, and these apparently form themselves into urea in the normal state, and in the abnormal state into uric acid and occasionally carbonate of ammonia. This, however, chiefly takes place in circumstances in which either much nitrogenous matter is conveyed into the system, or the oxydizing processes of respiration and circulation are not perfectly accomplished.

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* Journal de Medecine. Juin 1786. P. 476.

The conclusions at which the author arrives on this point are contained in the following sentences.

"The result of these considerations is, that I cannot look on the disappearance of urea and uric acid in the urine, and their accumulation in the blood, as anything else than a frequent symptom and consequence of gout, itself again being the cause of other important phenomena. The kidneys are obviously the principal emunctories of these substances in the healthy state of the system. If their function be arrested, either the suppressed urea and urates must be eliminated through some vicarious channel, or be retained in the current of the blood, in which latter case we observe the most poisonous and even fatal results. This is indeed the frequent cause of that general cachectic condition so often seen in gout. Hence arise distressing headaches, somnolence, and indifference. It is also, I believe, the origin of much of the dyspepsia with which the gouty are afflicted; and to this circumstance are to be traced those disastrous cases in which we see sudden death resulting from some great emotion, or from some violence affecting any great function of the body. The sudden suppression of urea and urates in the urine, and the great increase of these matters in the blood, make a case tantamount to poisoning. It scarcely admits of a doubt that a great mental shock, a violent catharsis, or indeed any great cause of depression, has the effect of arresting for a time the function of the kidney. It is not the case in gout alone; it is even more remarkable in hysteria."—Pp. 76, 77.

Discarding in its present state, for the reasons now assigned, the hypothesis that the presence of uric acid within the system causes gout, Dr Gairdner modifies this hypothesis, according to principles which he believes are better founded, and admit of more perfect application, to the phenomena of the disease. Allowing as established the following facts; that urea, uric acid, and hippuric acid, though derived from the food, are not received with it, but formed within the body, and enter the blood in the early stages of assimilation; that they are constant and necessary ingredients of the blood; and that, nevertheless, we know not any useful purposes which they serve, and can only consider them in the light of refuse or effete matters, which if not duly eliminated, produce much disturbance to health;—he then considers what effect the altered relation of these substances to each other would have, in accounting for the phenomena of gout.

In this inquiry, of course, hippuric acid, as pertaining only to the lower animals, is placed out of the question, and attention is directed solely to urea, and uric acid.

In certain states of the system, Dr Gairdner infers that urea, as it is in process of formation, becomes uric acid. He assigns various reasons for departing from the view taken by Liebig and Wohler, that urea is formed from uric acid. From the facts of two cases, which are given in detail, he infers that uric acid, if not derived from urea, at least has the same origin, and that these subtances are vicarious of each other. In a state of health, he argues, the elements necessary for the composition of urea, are separated from the blood. Under the influence of the gouty diathesis, however, the secretion of uric acid takes place in greater abundance. If it be attempted to present this inference in another form, it may be said that the nitrogen, hydrogen, oxygen, and carbon, which in the state of health, not being employed in nutrition, form urea, in deviations from health, arrange themselves into uric acid.

To show the relations of various nutritious and excrementitial elements, Dr Gairdner presents a table founded on analyses given in Dr Turner's Chemistry, and in which two atoms of nitrogen are assumed a common point of comparison between the articles.

	N.	H.	O.	C.
Urea, ...	2 ...	4 ...	2 ...	2
Uric acid, ...	2 ...	2 ...	3 ...	5
Hippuric acid, ...	2 ...	16 ...	10 ...	36
Fibrin, ... Albumen, ... Casein, ...	2 ...	12 ...	5 ...	16

It appears here, that in all these substances, the proportion of nitrogen being the same, uric acid contains less hydrogen, but more oxygen and carbon, than urea, while fibrin, albumen, and casein, each contain greatly more hydrogen and carbon, and more than double the amount of oxygen. Dr Gairdner, therefore, infers, that a less perfect consumption of carbon, or elimination of carbonic acid, in other words, a less perfect degree of oxygenation, leaves uric acid in the blood, instead of urea; and hence, he thinks, may be explained the influence of indolence in the production of this symptom of the disease, as also the vicarious origin of urea and uric acid, without having recourse to the idea of Liebig, that they reciprocally may be converted into each other at a subsequent stage.

To the function of respiration, therefore, Dr Gairdner looks as the main agent by which these transformations are effected in the nutritive materials conveyed into the blood. To constitute uric acid, he observes, the principal change required is the addition of carbon to urea, that is, carbon added to the elements which should form urea.

> "Physiologists are now agreed that, though the breathing be carried on in the lungs, and the changes of the blood, most obvious to the eye, take place there also, yet the arterialization is completed in the systemic circulation. The oxygen, which is absorbed into the blood in respiration, is slowly and gradually combined with it, effecting those changes necessary to its uses in, and diffusing warmth to, the whole of the body. During the progress of the blood to the veins, it receives the carbon thrown out of the system, and becomes venous, in which state it is carried forward to the heart to undergo the process of renewal, and be fitted again for the purposes of life.
>
> "But if, during respiration, the above changes be imperfect, all the living process I have described is disturbed. In the history of gout I have pointed out many signs indicating that the decarbonization of the blood is deficient. The blue lips, the swollen veins, the bloated complexion, the skin diseases, and general cachectic condition, sufficiently mark this. It is indeed evident in the whole natural history of the disease.

"When the blood is perfectly oxygenated, the heart and vessels are roused to energetic contraction; but it is quite certain that, when the expiration of carbonic acid has been less copious, the absorption of oxygen diminished, and a portion of venous blood carried forward into the arterial system, the circulation is slackened, a depressing influence is exercised on the brain and nervous system, congestion takes place in the vessels, and that state of things which I have pointed out as favourable to the creation of a gouty diathesis is speedily brought about.

"The effect of this condition on health must be obvious to every one; but its relation to the habits of a gouty patient is not so apparent. To this I wish pointedly and earnestly to call attention. In it there is much matter of interest, and matter directly applicable to the treatment of gout.

"The quantity of carbonic acid exhaled from the lungs is subject to great variation. By exertion it is much increased; in repose, its amount is comparatively small. In sleep it is yet further diminished; and during the long torpor of hybernating animals, it is diminished to such an amount, as under muscular exertion would be incompatible with life.

"We shall find a condition in some degree analogous in the heavy breathing of sleep and in the retarded and less deep inspirations which take place at seasons of repose, or in a life of sluggishness and bodily rest. In asphyxia, whether arising from drowning, strangulation, or the inhalation of mephitic air, a struggle is immediately caused on the part of the heart and arteries to drive forward the interrupted current of blood; but the irremovable impediment in the lungs causes stagnation in the veins, and congestion of all the capillaries. This is quickly followed by prostration of muscular power. It seems quite certain that muscular activity is incompatible with interrupted respiration; and indeed there is much reason to believe that not only muscular activity, but muscular power and a well-nourished muscular frame, bear a direct ratio to the capacity of the chest, and the perfection of the function of respiration. The observations, indeed, of modern physiologists have proved that the contractile muscular power is entirely independent of the influence of the brain, and have revived the doctrine of Haller of a *vis insita*. But though the contractile power rests on influences resident in the muscle itself, all observations prove its great dependence on the function of respiration.

"Not only is the expulsion of carbonic acid favoured by exercise and pure air, but it is much affected by the condition of the patient's mind. Vacuity of thought and cheerfulness of spirits aid its elimination; when combined with exercise and air, they lead to deep and powerful inspirations, and to great improvement of health. On the contrary, intense and long-continued employment of the mind, especially if combined with anxiety and apprehension, and a stimulating diet, are always attended with imperfect oxygenation of the blood, and deficient elimination of its carbonaceous matters.

"But it has already appeared to us probable that the urea and uric acid, which are constant and healthy ingredients of the blood, take their origin at this particular place and time, viz. in the lungs, during the process of respiration. It appears also certain that, in some conditions of the body, and with some habits of the patient, the carbonaceous matters, which should be cast out of the system, are retained in the blood. Can it then be wonderful that, of two ingredients, the one which is most abundant in carbon should now be formed at the expense of that which contains less; that the urates, in fact, should be found in excess in the blood and in the urine, and the urea should suffer a very material diminution?"—Pp. 91-95

Dr Gairdner further finds, that oxygenation of the blood is less perfect during the heavy sluggish breathing of sleep, than while the lungs are expanded in exercise; that the effect of repose is to increase the quantity of urates and diminish the amount of urea; while under the use of exercise the proportion of urates is diminished, and that of urea increased to what is conceived to be the natural standard. The effect of diet also deserves notice. According to Lehmann, the natural ratio of urea to uric acid under the use of mixed healthy diet is that of 27 to 1; under a diet purely animal, the urea became increased, so as to present the ratio of 32·7 to 1. Under the use of a vegetable diet again it fell to 22 to 1; and under a non-nitrogenous diet it was 20 to 1.

In short, the conclusion at which Dr Gairdner arrives is, that in gout not merely an increase of urates in the excretions takes place, but the relation of the urea to the uric acid is altered; and he infers that this change takes place in the early stage of the assimilation of the food during the process of respiration.

Dr Gairdner accordingly regards gout as a distemper to which inflammation is not essential; and, in accordance with the tendency which the sentiments of physicians are at present evincing, he thinks it is more judicious to rank it among cachectic than among phlegmasial diseases. He disapproves also of that part of the theory of Dr Prout, according to which the tuberculous and the arthritic diathesis are associated. He thinks, on the contrary, that observation entitles him to say, that the gouty have a singular immunity from other constitutional diseases, as tubercle, scrofula, and cancer.

In the further explanation of the peculiar nature of this distemper, he attaches great importance to the varicose and distended state of the veins, which he thinks indicates that the heart is oppressed by the stream of returning venous blood; and this, with the impure condition of the fluid from the non-elimination of urea and the urates, and probably the biliary constituents, he regards as the cause of those symptoms of disorder in the functions of the heart, which he mentions as an early indication of the approach and presence of the disease. Venous congestion, therefore, he considers as the first condition essential to the formation of the gouty paroxysm (p. 122). This may occasion in the small vessels of the extremities either hemorrhage and ecchymosis, or œdema. All these conditions, however, are the effect of too copious an assimilation of nutriment, of defective respiration, and more or less suppression of the healthy evacuations from the liver, the kidneys, and the skin (p. 146). The plethoric state thus engendered causes in strong constitutions painful manifestations of the regular disease; in feeble habits its irregular and atonic forms. This plethoric condition, again, has its cause in various indulgences, the nature and effect of which Dr Gairdner also indicates as distinctly as is requisite in a medical treatise. On another question, however, why certain constitutions when thus affected generate gout, while others evince no tendency to the disease, he confesses that neither scientific observation nor experience has hitherto been able to throw any light.

The views now given on the origin and nature of gout are employed to establish the principles of treatment.

Dr Gairdner entertains no doubt of the perfect curability of the disease in its early stages, not indeed by mere physic, but through the medium of the physician. He thinks, however, that the arguments already stated show, that this cannot be effected by mere starving, which, instead of curing, will rather aggravate some of the most serious forms of gout. It is from a judicious combination of dietetic, hygienic, and medicinal measures that proper results are to be expected to proceed.

In his subsequent observations on the influence of diet, and on the management of the diet of children and adults, Dr Gairdner allows, that under abstinence cures of gout have been repeatedly observed, and that, consequently, a moderate and even abstemious mode of living may in certain cases be advantageous and beneficial. He nevertheless maintains that, from all the facts which he has adduced, gout is not to be cured nor even benefited by a very low scale of diet. Dr Gairdner is of opinion that, notwithstanding the great number of physicians, by whom at all times the treatment by means of starving, as he calls it, has been inculcated and prescribed, yet is it a practice productive of injury; and so far is it from being correct, that it is beneficial, that all real nourishment, he maintains, contributes to the cure of the disease. Food he recommends to be taken for the construction and maintenance of bone, muscle, and we suppose tendon, ligament, and synovial membrane. Dr Gairdner nevertheless allows, with all this censure of low diet, that "though all increase of strength and power is most salutary in gout, yet everything that oppresses the system in the form of superfluous fat or redundant circulation is most pernicious" (pp. 161 and 168.)

Upon this last concession, which is made in two different passages, depends, we conceive, the determination of the whole matter of the species of food and regimen, which is most advisable in the treatment of gout. The question, in truth, which Dr Gairdner here introduces, is in our estimation a dispute about words. The whole question depends on what is meant by low diet, and what the author by rather a strong term denominates starving. As to the latter, we are not aware that many physicians have recommended starving, properly so called, as a remedy in gout. By starving is meant, according to our conception, so entire a denial of food, so complete a withholding of the means of sustenance, that, if continued, it must of itself terminate in death. The original German, STERBEN, from which the Anglo-Saxon STARVE is derived, means to die; and it requires little lexicographical talent to show, that starvation means either the total, or very nearly the total, want of food; or to be so completely deprived of food, that if the privation be continued, death would ensue. Now, we must really be permitted to doubt whether any physician in his senses ever recommended a patient in gout to be starved. Moderation in different degrees, and abstemiousness, he might recommend. But this means exactly what Dr Gairdner admits in various parts of his treatise to be right, and indeed the judicious course. It is not what the patient eats, it must be remembered, that contributes to nutriment and strength, but what he digests, that is, what his assimilating organs apply properly to the maintenance of the tissues.

"We all consume," says Dr Gairdner, "much more food than is necessary for the sustenance of the body in health and strength. There is no necessity that persons, without any proclivity to disease, should confine themselves within the strict limits of the absolutely essential; for the human body has a great power of adaptation, and a range is permitted, within which the pleasures of the table may be enjoyed without injury. To the gouty, however, no such licence can be given. If they desire to escape the penalties of this sad disease, they must eat only for the purposes of life, and with a view to health."—Pp. 131, 132.

This is exactly what we understand judicious physicians have in general recommended,—such a degree of moderation in the use of articles of food, as may sustain, yet not oppress and induce or continue the plethora, which Dr Gairdner himself allows to be one of the principal characters of the distemper.

Of milk diet Dr Gairdner disapproves as inadequate to the expectations usually entertained of it. A diet purely farinaceous or vegetable is, he thinks, still less to be recommended. He states that he has more than once seen it attempted in gout, and, according to his observation, always with great aggravation of the patient's sufferings. A perfectly vegetable diet never can, he thinks, be reconciled with a life of leisure and ease. The experience of the world is against it. Though some persons can undergo great mental labour on a vegetable diet, this is the exception to the rule. The more common case is, that men who lead a life of mental application and anxiety, must have a diet more or less animal. The most suitable regimen for the gouty he regards a mixture of animal and vegetable food, in which the former greatly predominates, but which is to be administered in a manner so frugal, that it may be within the power of the stomach to digest and of the constitution to assimilate.

The *cure des raisins*, so much recommended for the removal of various chronic disorders in France and Switzerland, he represents to be quite inefficient; and as Dr Gairdner speaks from personal observation, his testimony is entitled to attention.

As to the use of the vinous and other factitious drinks, Dr Gairdner gives it as the result of whatever information he could obtain when abroad, that, in the wine countries of France, the inhabitants are less subject to gout than the people of England. The inference from this, is, that the lighter sorts of French wines, as the different species of claret, Burgundy, and other products of these provinces, are, when used as the inhabitants use them, not so productive of gout, as the stronger wines of Portugal, and those which are usually called English; that is, we understand, prepared for the English market. Port wine, which in truth is a most pernicious and poisonous liquor, he regards as the only wine which has a sure, rapid, and pernicious effect. It may be reasonably asked, indeed, whether the name of wine can be justly applied to a compound, of which four ordinary sized wine glasses contain one full glass of spirits of wine. The use of brandy and water Dr Gairdner equally pronounces to be most hurtful to the gouty.

On the subject of the strictly medical treatment, the instructions of Dr Gairdner are of the following nature.

Blood-letting as a general measure is much to be avoided in the treatment of gout, and the large blood-lettings required in the pure *phlegmasiæ* are completely inadmissible. The blood is loaded with foreign matter, the urates, and in some sense poisoned by these articles; and blood-letting to a large amount not only does not remove this poison, but it renders the system less capable of struggling against their effects.

A more suitable form of this evacuation consists in the use of small blood-lettings, of from three to six ounces, repeated according as the symptoms demand, and as effects are produced. Small blood-lettings, Dr Gairdner thinks, produce a strengthening effect by their operation in removing plethora. The small blood-lettings are also useful in superseding the use of purgatives, which he maintains are hurtful in gout, and attended with considerable risk, when used after the manner recommended by Dr Sutton, for the relief of the febrile symptoms by which the disorder is attended.

Dr Gairdner takes a careful and judicious survey of the opinions of physicians on the use of purgatives, in the treatment of the gouty diathesis and the gouty paroxysm. Though he disapproves of the constant employment of these agents in gout, of the use of drastic purgatives, and of purgatives as mere means of diminishing plethora and febrile heat, he does not entirely exclude their employment. Laxatives he regards as essential, even where patients live abstemiously, in order to relieve the bowels occasionally. The ordinary vegetable aperients, as senna, rhubarb, aloes, jalap, and scammony, he regards as best adapted to the constitution of the gouty.

The neutral salts he looks upon as inadmissible, in consequence of the profuse watery evacuations which they usually produce. But given in small doses, as diuretics, he has found them advantageous. The most eligible are those formed by the vegetable acids and by phosphoric acid, especially the latter, as the tasteless salts.

To the employment of the alkalies and the earthy carbonates, Dr Gairdner is opposed. The view of the nature of the disease which he gives, must show, he thinks, that these medicines have been praised far beyond their actual merits. Under their use he maintains that the uric acid of the urine never disappears, or is greatly diminished; and he states that he has seen it vanish under the employment of the neutral saltsalready mentioned,—for instance, the phosphates, the tartrates, and citrates of potass and soda,—after it had resisted all change under the use of the carbonates.

It is unnecessary to notice here what is said of the use of the vegetable bitters recommended as tonics.

In one form of the disease, which, according to the author, is seen chiefly in aged persons, and which is manifested by swelling of the articulations of the hand, with slight derangement of stomach, faltering in the action of the heart, and intermission of the pulse, Dr Gairdner looks upon iron as the best, if not the only remedy. The forms of the mineral of which he has most experience, are the saccharine carbonate, the citrate, and the tartrate, of either of which from five to ten grains may be taken twice daily in any aromatic water, and followed occasionally at night by eight or ten grains of extract of rhubarb with one drachm of phosphate of soda and half an ounce of compound decoction of aloes.

Meadow-saffon he recommends as an excellent remedy if given in moderate doses, and so as to produce neither griping, purging, nor other disorder of the alimentary canal. In short, this medicine, he thinks, never more effectually relieves the patient than when it acts in what is termed a silent and peaceful manner,—a view which is that now taken by many physicians of judgment and experience. It must be remarked, however, that when meadow-saffron is said to operate in this silent and peaceful manner, this representation refers only to its primary action as an irritant of the alimentary mucous membrane. While it is thus believed to be acting silently, it operates energetically though slowly on the kidneys as a diuretic, sometimes upon the skin as a gentle diaphoretic, and, after the blood has been charged with it, upon the liver. Dr Gairdner thinks that the proper action of meadow-saffron, administered in moderate doses, is that of a narcotic (p. 202). This may be the case; but no one, who has had much experience in the administration of this drug, if he has carefully observed its effects, will refuse to admit its influence on the kidneys and on the circulation of the skin.

The remedy, Dr Gairdner adds, is most efficient in cases of open regular gout; if there be local disorder, or much congestion of an internal organ, this ought to be removed by suitable means. He is perfectly right in recommending small doses, from ten to fifteen minims, of any of the vinous solutions, twice or three times daily, as the most eligible form of administration.

On diet, exercise, and the use of fresh air, Dr Gairdner delivers useful instructions.

In short, since in gout an imperfect elimination of carbon takes place, this must be counteracted by placing the patient in situations where he may obtain large supplies of fresh air, in order thoroughly to oxidate the carbonaceous matter; and exercise must be regularly taken, so as to compel, as it were, the lungs to decarbonize more efficiently the blood.

In the foregoing sketch, it has been our study to communicate a just account of the speculative pathological principles given by the author, and the therapeutic instructions thence educed, without expressing any opinion on the merits of the former, or the propriety of the latter. After giving this view of what the treatise contains, it is best to leave to readers in general, to form an estimate of the value of the essay. We avoid in general the language of panegyric, and deal still less in that of censure. The former is liable to be insincere, the latter to be unjust; and both to be indiscriminating. It implies not a departure from this principle to say, that this essay shows that the author both observes for himself the phenomena of disease, and reasons on them, with ingenuity and originality; and the volume contains many suggestions, both speculative and practical, which entitle it to the attention of physicians.

Art. IV.—*On the Cryptogamous Origin of Malarious and Epidemic Fevers.* By J. K. Mitchell, A. M., M. D., Professor of Practical Medicine in the Jefferson Medical College of Philadelphia. Philadelphia, 1849. Pp. 137.

The denomination Fungous Origin, and Fungous Hypothesis of Diseases, to which attention has been recently solicited, is not fortunate. It is, indeed, what ancient writers would call inauspicious, in so far as it gives occasion to those inclined, to think that the hypothesis is as unsubstantial, and as fleeting and temporary in duration, as the vegetable productions from which it derives its appellation. Certain it is, that scarcely anything could be less durable than the hypothesis by which it was attempted to explain the origin of cholera, by ascribing that disease to the presence and operation of fungous growths.

It may or may not be, that in order to obviate this misapplication of the term, that the present author, Dr Mitchell of Philadelphia, has selected the less pliant and flexible name of cryptogamous, in his undertaking to throw some new light upon the origin of miasmatic and epidemic fevers. Though the name is different, the signification is the same. Dr Mitchell thinks that various facts show that, the true causes to which the origin and prevalence of endemic and epidemic fevers have been attributed, are neither the miasmata of marshy grounds, nor the emanations from the soil, but the cryptogamous plants, *fungi*, or mushrooms of various species, which are abundantly produced in situations where, and during seasons when, fevers of this order prevail. On the success of this undertaking, different opinions may be entertained. But it must be conceded, that Dr Mitchell has examined with great learning and judgment, the circumstances in which this order of febrile distempers is liable to be produced, and has adduced a body of evidence which shows, that the hypothesis in his hands is entitled to considerable attention.

Dr Mitchell examines first the usually received theories on the origin of endemic fevers, and considers with candour and impartiality the evidence for their truth. Every one is aware that the theory or hypothesis which ascribes these diseases to what are called marsh miasmata and telluric emanations is open to various objections; and all the progress which impartial inquirers, not decidedly attached to one side, have been able to make, is to infer that the hypothesis, after being modified and restricted by various conditions, possesses a certain degree of probability, and is chiefly adopted, because there is none other which possesses stronger and more manifest claims on the attention of the medical profession. It must be further allowed, that though its reign has been upon the whole lengthened, yet the experience of every season, and every cycle of seasons, furnishes facts, of which it does not in all instances present an easy explanation. Under these circumstances, it is not difficult to understand that this theory may require to be modified by the restricting power of additional conditions.

In the following passage Dr Mitchell states the difficulties by which this theory is allowed to be embarrassed in the writings of physicians, who have either espoused its defence or admitted its weak points:—

> "The commonly received marsh theory is well stated and supported by Macculloch, to whose work on Malaria I refer you for a view of the question. It is in a much more masterly and precise manner, sustained by Dr Craigie of Edinburgh, to whose volumes on the Practice of Medicine you may most profitably resort for a learned, lucid, and, I think, impartial array of the facts bearing on that side of the question. Macculloch involves himself in difficulties without seeming to see them; whilst Craigie, although inclined to the same conclusions, views with a master's eye the whole of the impediments and objections. The objections presented by the latter are: the low temperature at which these disease-producing changes may take place; the unaccountable production of them in places in which there is no apparent vegetation and often no marsh; the exemption of certain places where occur all the seeming elements of decomposition; the inexplicable effects of rural cultivation; and the unexplained vicissitudes of health in the same places in different though similar years."—P. 15.

It is well known, accordingly, that the hypothesis of Malaria and telluric emanations has at all times had opponents. Of these Dr Mitchell mentions two, namely, Professor Dunglison and Dr John Bell, who, nevertheless, have been more successful in showing the defects of the old hypothesis, or that which Dr Mitchell names the Vegeto-aerial, than advancing any new one would.

To any one who undertakes to investigate the evidence on which this hypothesis is to be considered as tenable, two questions at the fewest readily suggest themselves for consideration. The first is, Are there regions and districts in the habitable and inhabited globe in which the physical conditions usually specified as associated with the production of endemic fevers are present, yet in which fevers of this description are not observed to prevail? Are there regions and districts, in which there are marshes, stagnant pools, and ditches, or alluvial flats, with extensive subsoil, moisture and dry surfaces, or argillaceous and gravelly lands, in which endemic fevers are not observed to arise?

The second question is, Are there regions and districts in which fevers are endemic and occasionally epidemic, yet in which

neither marshes nor ditches are evident, nor alluvial flats with extensive subsoil moisture, nor argillaceous bottoms, nor gravelly banks, nor dry rocky ravines, are observed to exist?

Of such places as the dry sandy plains of Brabant, in which it is well known remittent and intermittent fevers are endemic, and in certain seasons epidemic, various authors have spoken as furnishing arguments against the miasmatic hypothesis; and we observe that Dr Mitchell adds his name to the number. Plains of the kind now specified, however, furnish no arguments of the kind stated. These plains, however dry they appear at the surface, are perfectly moist and watery, much like a sponge; and, in point of fact, when carefully examined, they furnish substantial arguments in favour of the hypothesis. Such plains are common in Belgium and Holland, and in certain parts of Spain; and in all instances, if perforations are dug, to a moderate depth, the cavities are speedily filled with water, which percolates, indeed, through the entire subsoil, to the depth of no one can tell how many feet. In Belgium, especially where the surface is flat and much intersected by slowly moving rivers, the subsoil moisture is most abundant. Now it is exactly this condition of subsoil that favours the production of the poisonous cause of the endemic fever, whatever that may be. As soon as the surface is exposed to considerable solar heat, on the approach of warm weather, that is, after the vernal equinox, a process of desiccation advances all over it for about one inch or so down. But along with this process, water is constantly ascending to the surface through the loose porous soil; and as that which ascends is evaporated by the heat, a constant process of exactly the same nature as that which takes place on the margins of the marsh, must be advancing at the points at which the subsoil moisture is in contact with the drying surface of mould. Soils and districts of the kind now specified are, to all intents and purposes, miasmatic. We do not pretend to say what the poisonous element is; but we think every accurate observer of these soils must allow that they possess all the conditions necessary to produce a poison, the same as that which is exhaled from the surface of the soil in districts confessedly miasmatic; for instance, the banks of the Scheldt in Holland; the mouths of the Rhone in France; the delta of the Mississippi, in America; and the delta of the Niger in Africa.

It is our opinion, therefore, that this case cannot be received.

The example of New South Wales furnishes a greatly stronger argument. That country, extending from 10°. 5′ to 38° S. latitude, embraces a region similarly situate to that of America, from the West Indies to Chesapeake Bay. It is subject to a rainy season; it has streams, estuaries, and extensive swamps. Around some of its towns lies a deep black highly productive vegetable mould. It is liable to extraordinary inundations, which lay the country, as far as the eye can reach, under a sheet of muddy water. The temperature is as high as that of any other like latitutude. The coast is covered by mangroves. Its animal and vegetable products are those of the equinoxial regions. But notwithstanding all these conditions, which usually denote a febriferous region, New Holland is remarkable for its salubrity. Pulmonary diseases, and in the wet season dysenteric attacks, are observed; but the fevers incident to warm climates elsewhere are of rare occurrence in this part of the globe.

Mr Titian Peale, who accompanied Captain Wilkes in the exploring expedition to the Pacific Ocean, states, that he never saw a case of ague, either in natives or strangers in the Polynesian Islands, though the officers and men employed lived and slept in the midst of marsh exhalations and mosquitoes, when the days were hot, and the huts were open and exposed.

The experience derived from the Island of TONGATABOO is of the same nature. This is what is called an organic island, formed by coral; flat, rich, and luxuriant, with a temperature rising to 98° F., and offering a mean temperature during the period of the sojourn of the expedition of 79°, 25′. Much rain fell, and when the atmosphere was clear, there were heavy dews. The people of the expedition, though on shore at night, suffered no attack of illness.

OVOLAU (FEGEE) is a volcanic island, the mean temperature of which, for six weeks, was 77°, 81′; maximum, 96°; minimum, 62°. Vegetation was vigorous and rapid. Fevers are unknown.

The island of Singapore in the East Indies, is on the main low and level. In the interior rises one hill to the height of about 500 feet, and in this granite rocks make their appearance. Scattered here and there are low round sand-hills, the level ground between which is formed of a ferruginous clay upon a sandy substratum. The greater part of the island is covered with jungle. In many places are found lofty trees, and a most luxuriant vegetation. The island is well watered by streams, which descend from the hills to the sea. The tides have produced and continued a chain of marshes nearly all round the coast. In some places fresh stagnant water covers extensively the low grounds.

The city, which lies in latitude 1°. 17′, that is, almost, under the equator, contains a mixed population of Chinese, Malays, Indians, and Europeans, amounting to about 20,000 souls. It is nearly surrounded by marshes, the jungles of which are almost impenetrable, and are infested by tigers and other ferocious animals. In some spots the Chinese have cleared and cultivated the ground, and near the city are some sugar and nutmeg plantations.

Vegetation is rapid and profuse; decay is not less rapid. The mean annual temperature is, in the morning and evening, 79°.45; at noon, 84°.

Notwithstanding this combination of physical circumstances, fevers, especially among the natives, are rare. Remittent fevers take place occasionally; agues more rarely. Foreigners are the parties most readily attacked; and attacks, though few, are attributable chiefly to exposure to the sun, or much fatigue. The island has the character of being very healthy.

We have only to observe, that, with regard to the physical characters of the soil of Singapore, wherever there is ferruginous, that is, iron-clay, endemic disease is almost unknown. With the ferruginous impregnation, the production of endemic or telluric disease seems to be incompatible.

The author next refers to Brazil, the physical and hygienic characters of which being generally known, it is unnecessary to repeat.

The hypothesis proposed by Mr Daniel regarding the influence of sulphuretted hydrogen, he regards as quite inadmissible, in so far as in situations where are sulphur-works and manufactories for gunpowder and sulphuric acid, endemic fevers prevail so little, that he states that ague, with vegetation, has entirely disappeared. Nor does Dr Mitchell assign greater importance to other gaseous bodies, to which different speculators have attributed the origin of remittent fever.

After disposing of the miasmatic hypothesis, as modified by Dr Fergusson and Dr Jackson, he directs passing attention to what has been called the animalcular hypothesis, or that which ascribes endemic and epidemic disorders to the presence of multitudes of minute animals. Against the truth of this hypothesis, he urges the following objections:—1. It has never been shown that animalcules are in any way poisonous. 2. None of the difficulties of the inquiry are thus removed. 3. The assumption is hypothetical at first, and does not, in the progress of examination, become more demonstratively probable or logically admissible. And, *4th*, To the vegetable branch of the organic hypothesis belongs a higher degree of probability, and, by adopting it, much of the obscurity of the subject may, in the estimation of the author, be dissipated.

Upon the exposition of this hypothesis, therefore, namely, that by the germs of cryptogamous plants, Dr Mitchell enters. The idea of this hypothesis appears to have been suggested to his mind by accident; though, after its first suggestion, he appears to have diligently collected proofs in favour of its probability.

Standing, Dr Mitchell informs us, at St George's, in Delaware, more than twenty years ago, upon the bottom of what had been a short time before a mill-dam, he found around, the undecayed stumps of several trees, which had been for 117 years submerged in fresh water. Two or three years afterwards, when he again visited this spot, he observed that these stumps, no longer thoroughly covered by water, but damp, had been entirely disintegrated by the dry-rot, and that they crumbled to pieces in handling. In the handful of dust which he picked up he found innumerable spores of what he supposed to be POLYPORUS DESTRUCTOR, and MERULIUS VASTATOR, cryptogamous plants, whose active existence had been bought, he adds, at the expense of the old stumps. Dr Mitchell immediately drew the inference, that probably the miasm, so much dreaded in that place, might be directly or indirectly the product of these concomitants of rapid decomposition. He has since that time collected many facts which he thinks tend to give the hypothesis a degree of probability. At the same time he candidly acknowledges, that though he has much confidence in the force of his subject, he has no desire, that the hypothesis be established at the expense of truth and reason.

After the general statement of the hypothesis, Dr Mitchell adduces those facts and arguments which he thinks tend to show its probability, if not to prove its validity. It is unnecessary that we follow the author in this survey. We shall merely advert to some of the leading topics in the train of reasoning.

Among cryptogamous plants the FUNGI are the most remarkable. Occupying an intermediate position between the LICHENS, which thrive in dry soils, and the ALGÆ, which live in water, salt or fresh, they evince a preference for damp, unsound, and loaded atmospheres, and feed on organized matter, the vitality of which is gone or going.

To this condition it appears to us that the fungi constituting fairy rings form an exception. The air there is neither damp nor unsound. It is of the dry, pure, and to all appearance, most salubrious.

In all the FUNGI the element is a minute cell, not often distinguishable when isolated from the elementary cells of even animal organisms. Of all vegetable substances they are, chemically, most highly animalized. Like animals, they evolve carbonic acid and imbibe a quantity of oxygen; from some, hydrogen, and from some, nitrogen, is extricated. They yield the vegetable products resin, sugar, gum, fungic acid, and saline compounds; and they afford adipocire, albumen, and osmazome, as animal elements. Their basis is fungin, a substance white, soft, and doughy, which yields nitrogen, hydrocyanic acid, oxalic and some other acids, and wax, tallow, and, in some instances, oil. According to the author, they are chemically constituted plants, some with animal motions and volitions, and others with animal composition and the structure and habitudes of vegetables.

It is unnecessary to follow Dr Mitchell in the enumeration of the various beings, animal and vegetable, which are liable to be attacked by fungi of various sorts and in different parts. All this has been so fully done in previous numbers, that it would only lead to useless repetition. It is sufficient to observe that, in the decomposition of all dead animal and vegetable matter, and their aqueous solutions, that is to say, wherever water or moisture comes in contact with animal or vegetable matter, fungi are formed. Thus clothes, leather, timber, in damp situations, become covered by fungi of various sorts. Dr Mitchell shows that the insalubrity of any given region or district has the most constant relation to the habits of the living vegetation. Whatever may be the temperature or humidity, the most unhealthy period of the year is in any given locality that, when the phanerogamous vegetation has completed its annual task of growth, flowering, and fruitage, and feeling the weakness of an exhausting effort, it is liable to be overcome, as it were, by the cryptogamous plants which now begin upon the phanerogamous division their operations of plunder and destruction.

The fungi are chiefly active in the end of summer and autumn; and a wet autumn is generally found to be prolific in these plants. Even when forced to prematurity in May and June under the influence of heavy rains and unseasonable heat, they are never so perfect or vigorous, as they are when produced in what may be regarded as their proper season. Of one hundred and five species of fungi enumerated by M. Roques, only one grows at all seasons; four in spring; one in spring and autumn; three at the very end of summer; eight exclusively in summer; twenty-eight in summer and autumn; and sixty-two exclusively in autumn. Of the total number, therefore, of one hundred and five, ninety-two are active in autumn, and thirty-six in summer.

This survey leads to the inference, that the growth of *fungi* and the formation of Malarial Fevers is in general, in the order directly of,—autumn, summer, spring, winter.

Dr Mitchell next directs attention to great frequency of attacks of endemic fever during the night. Taking this fact as admitted, and as showing the superior liability of the frame to receive the poison of these diseases, and the greater power of these diseases to attack, during the cold dark hours of night than during the day, he maintains, that all the explanations hitherto suggested of night air, low temperature, exhalations being more energetic after nightfall, than during the continuance of daylight,—are quite inadequate to account for the facts. On the other hand, when we advert to the tendency which fungous plants have to be developed and spring up during the night, and to show their general energy, we must allow that an important analogy between the prevalence and energy of malaria, and the growth and activity of fungi is thus established.

It may here be proposed as a question, whether the exhalations from these plants are not more active or derive greater energy from the moist air of night, loaded as it is with dew and humid vapour, than from the dry warm air of day.

Dr Mitchell adduces next various instances of epidemic diseases, during the prevalence of which *fungi* in various forms, as rust, smut, mould, mildew, bloody rains, and similar unusual phenomena, were abundantly produced.

The milk-sickness, trembles, tires, slows, stiff-joints, and puking fever, are different names for a peculiar distemper incident to the cattle of the western regions of the United States. This distemper prevails mostly in the end of summer and in autumn, chiefly in September and in October. It acts only at night, or until the dew has been evaporated from the grass in the morning. It prevails in rich alluvial pastures, on high barren ridges, on open plains, and in the deepest woods. Dr Mitchell infers from many concurring circumstances that the disease is owing to the presence and operation of *fungi* in the places frequented by the cattle. These *fungi*, he thinks, must be swallowed with the food, and in this manner give rise to the disease.

Dr Mitchell next adduces various facts to show that fungi, either when eaten or when introduced with food into the system, or when inhaled in the form of vapour, give rise in the human body to the symptoms of intermittent and remittent fever, of plague, or gangrenous fever, and other disorders usually ascribed to the influence of miasmatic poison. He next adverts to the fungous growths in the bodies of men and animals in connection with the cutaneous and mucous diseases, which have of late years attracted attention.

Fungi may also produce diseases quite similar to yellow fever. The fungi from which this disease arises, may be preserved in their sporules for months, or over several seasons, he thinks, and then, in favourable seasons, induce the disease. The same may be said of cholera, the propagation of which, he thinks, is most easily explained by this hypothesis.

Again, countries most remarkable for the prevalence of endemic fever, are also remarkable for the production of fungi. Thus, in Africa, whether in the interior, on the banks of rivers, or on the coast at their outlets, fungi are most easily produced. Every thing animal and vegetable is speedily covered with mould, especially during the rains. In Italy, the Maremma which is most febriferous is also most productive of mushrooms. The volcanic tufa round Naples, and that round Rome, are most remarkable for the abundnce of mushrooms they produce; and the same places, it is known, are most productive of fevers.

As a final argument to prove analogy and alliance between the production of fungi and the appearance of endemic and epidemic fevers, Dr Mitchell remarks, that these distempers are observed to possess periods of repose. After remittent fever has prevailed, one, or it may be two seasons most extensively in a district, it ceases for years. In the same manner, he observes, the growth of fungi is liable to undergo periods of inactivity. The volcanic tufa, if actively employed, loses, in about three or four years, the power of producing mushrooms. After the fungi have been for several seasons abundantly produced, they evince the property of destroying themselves. In other words, they exhaust the soil first, and then they die out; while a vigorous crop of grass, and healthy phanerogamous plants follows, as in the instance of fairy rings.

The foregoing is rather a hasty sketch of the contents of this volume. But it may serve to convey to the reader some idea of the manner in which the author treats his subject, and of the nature of the facts and arguments which he adduces in defence of his hypothesis. It is not to be expected that he will convince all. But he has the merit of directing attention to an important concomitant of febriferous seasons; and if he does not supplant the hypothesis of marsh miasmata and telluric exhalations, he at least shows cause why it ought to be modified.

It must also be allowed, that the cryptogamous hypothesis, as here stated, does not exclude the operation of marsh miasmata. It may be that the miasmata are the cryptogamous products. In other respects, the hypothesis makes a near approach to that of the ligneous hypothesis, so ingeniously explained in 1827 by Dr John Wilson.*

Finally, if there be merit in this hypothesis, it is due to Dr Mitchell to say, that he preceded Dr Cowdell in bringing forward this doctrine. The six lectures of which this work consists, were delivered at the Jefferson Medical College of Philadelphia, in the course of 1846.

ART. V.—*An Appeal to the Public on behalf of Sick Children.* London, 1850. Post 8vo, pp. 16.

IT is a fact well known to those who study the comparative mortality of the human race at different periods of life, that a large amount of sickness and death takes place during the first nine or ten years. The reports of the Registrar-General show that 24 per cent. of children die during the first two years, and nearly 11 per cent. during the ensuing eight years; so that 35 per cent. at least, or more than one-third, of the children born in London, die during the first ten years of life.

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* Memoirs of West India Fever, &c., by John Wilson, M. D., London, 1827; and Edinburgh Medical and Surgical Journal, vol. xxix., p. 189. Edin. 1828.

It may be safely assumed, that of the remaining sixty-five children, if not the whole, at least nearly the whole, have been several times ill by one or more of the disorders liable to attack that early period of life.

It could be easily shown that, of the large amount of mortality above mentioned, by far the largest proportion falls upon the children of the poor and least comfortable classes. It would be erroneous, no doubt, to represent the 35 in the hundred as all the children of the poor and more indigent families, and the 65 who pass through this period, as the offspring of those in more comfortable circumstances. Yet the fact is well known to medical practitioners, that to so great an extent does this mortality fall upon the children of the least comfortable classes, that it cannot be far from the truth when they are rated at about 27 or 28 per cent.

The diseases affecting infantile and early life are well known. Though vaccination has greatly curtailed the power of small-pox as a fatal disease, yet the life of the human infant is exposed to the assaults of fever, measles, scarlet fever, gastric and gastro-enteric diseases, hooping-cough, croup, bronchitis, pneumonia, affections of the brain, tubercular diseases affecting the brain, and a formidable array of diseases of the nervous system. Deaths by convulsions alone amounted, in 1846, to 2033, and those by hooping-cough to 2925.

Those who have had occasion to treat the children of the poor in their own homes well know how difficult it is to secure the necessary attendance and nursing, and to get carried into effect those attentions which are often of more importance than mere medical treatment, however necessary that may be.

A natural question is, Can this mortality be diminished? Are there any means by which the diseases of children might be rendered less frequently mortal?

To every one acquainted with the nature of these diseases it is well known that not only does much depend upon good medical treatment, seconded by attentive nursing, but much depends upon taking the diseases at an early period. This is often done by the medical officers of dispensaries. But all who are acquainted with the difficulties attending this sort of practice are aware that it is extremely difficult to get carried into effect the measures recommended.

The most effectual mode is doubtless an hospital for the reception of children, and children alone. With the view of supplying the want now mentioned, a number of benevolent persons, medical and general, propose to establish in London an hospital capable of containing one hundred beds for the accommodation of sick children between the ages of two and twelve years. No one can entertain any doubt that such an institution is both required, and, if established, would be the means of doing much good. The general hospitals are not fitted for the reception of these little patients; and though no city is better provided than London with hospitals for the adult population, we conceive that children would be better attended to if treated in hospitals appropriated to that age alone.

London possesses only one dispensary for the special treatment of the diseases of children; and neither London nor any other city in the British empire possesses any hospital devoted exclusively to the reception of sick children. Most of the principal cities of Europe already possess not only dispensaries for the treatment of the diseases of children, but many have hospitals for the more efficient management of the diseases incident to that tender age. Hospitals of this description are established in Paris, Frankfort, Hamburgh, Munich, Berlin, St Petersburgh, Vienna, Moscow, Prague, Pesth, Turin, Copenhagen, Stuttgart, Gfätz, Brünn, Lemberg, and even in Constantinople.

It is certainly reasonable to expect, that London, which is so liberal in institutions for the adult poor, should not be deficient in one institution at least for children.

One circumstance is obvious. It will not do to place a children's hospital in, or even near any of the densely-peopled parts of the city. Indeed, an hospital for children ought to be entirely beyond the precincts of the city, in some elevated, well-aired district, where the benefits of quiet and fresh air may be perfectly obtained. The building for such a purpose should on no consideration be erected either near the river or in the vicinity of houses, where the open space is likely to be contracted by the extension of the suburbs. It is premature at present to say which is the most eligible site. But, in all probability, the district which would combine the greatest number of advantages would be altogether to the north of the city and suburbs, either in the vicinity of Highgate or Hampstead, or in the line of that locality.

Another condition for which it will be requisite to make some arrangement is, that it will be well to make provision in many instances for the reception of mothers with their children. Though in many instances females in that rank of life understand little of the management of children, yet instances may occur in which infants and young children will be more easily managed with the aid of the mother than without.

All this, however, will come in good time.

In the meantime, a provisional committee, consisting of Dr Latham, Dr Burrows, Dr Ferguson, Dr Bence Jones, and Dr West, is appointed, in order to aid in carrying into effect the scheme proposed.

We trust that the proposition will obtain that degree of success to which, as a measure urgently required, it is most justly entitled.

PART III.

MEDICAL INTELLIGENCE.

I. PHYSIOLOGY.

Cause of Luminousness in the Glow-Worm. By CARLO MATTEUCCI, Professor of Physics, &c., in the University of Pisa. (Lectures on the Physical Phenomena of Living Beings, 1847, p. 162.)

The Glow-worm (*Lampyris Italica, Colophotia Italica;* Dejean; *Lucciola,* Italy; *Ver luisant,* France) is a coleopterous insect living in the grass, and which shows itself after sunset in spring and summer. The two last segments of its body, which by day appear yellowish, are slightly luminous in the dusk, and during the night evolve a bright intermittent light.

The light sometimes ceases suddenly, either when the insects are gently touched, or at times when they have not been touched, and subsequently reappears again.

This fact led Macaire to suppose, that the phosphorescence was under the will of the animal. The cessation of the luminosity is certainly not effected by an opaque membrane, which it was supposed the insect drew over its rings, for no such membrane exists.

Everything leads us to assume that the phosphorescence is not continuous, because the cause that produces it is not persistent; and that we can explain the intermittence of the phenomenon.

In studying this subject, the observation which excited the astonishment of M. Matteucci is, that the yellowish matter contained in the terminal rings of the insect continues to emit light when separated from the body of the animal. If we kill one of the insects and crush it between the fingers, long streaks of light are perceived to issue from the yellowish matter. The phosphorescence continues for a longer or shorter period, according to a variety of circumstances, which we shall presently investigate. This fact, indeed, proves that the integrity and life of the animal are not essentially necessary for its production. In order to study the matter, thus separated from the body of the insect, M. Matteucci commenced by examining what influence heat, electricity, and gaseous media had on it, as those persons had done who preceded him in this inquiry. At the same time, he studied the influence of the same causes on an entire and living insect; and thus, by comparison, adopted the most likely method of ascertaining the nature of the phenomenon.

M. M. placed several very lively and very luminous glow-worms in a glass tube immersed in water. A thermometer with a very small bulb was surrounded by these insects. He repeatedly endeavoured to ascertain whether the thermometer thus placed acquired, in consequence, a higher temperature, but he never observed that it did. By slowly heating the water, he saw the intensity of the light increase up to + 30° Reaumur. [= 99·5° Fahr.] At about this temperature the intermittence ceased, the light became continuous, and, by applying more heat, acquired a red colour. At + 40° Reaumur [= 122° Fahr.] the light entirely and finally ceased, and the animal died. By crushing the matter from the rings between the fingers it no longer evolved light.

In experimenting with the posterior luminous segments, instead of entire glow-worms, he discovered no difference in the phenomena. These results confirm the experiments made by Macaire on entire glow-worms, placed in water which was gradually heated.

Professor Matteucci exposed these animals both entire, and the luminous segments to the influence of cold, to an atmosphere of carbonic acid gas, to one of hydrogen, to one of atmospheric air, to an atmosphere of chlorine, to one of oxygen, to one of oxyhydrogen gas, to one of mixed oxygen and carbonic acid gas; he introduced mutilated insects into an atmosphere of pure oxygen gas; he placed the insects into receptacles of sulphuretted hydrogen gas, and into one of rarefied air; and he obtained results from which he deduces the following conclusions, which he regards as either new, or more correctly deduced than any hitherto published.

1st. The phosphorescence of the glow-worm may cease, without the insect being dead.

2dly. There exists in this insect a matter which evolves light, without any appreciable heat; and the animal, to manifest this property, does not necessarily require either to be entire, or to possess life.

3dly. Carbonic acid and hydrogen form a medium in which the phosphorescent matter of the glow-worm ceases to shine after an interval of time, not exceeding thirty or forty minutes, provided that the gases are pure.

4thly. In oxygen gas, the brilliancy of the phosphorescent matter is considerably greater than in atmospheric air, and the duration of the phosphorescence is nearly three times as long. This holds good with regard to the luminous segments only, as well as with the entire animal.

5thly. This phosphorescent matter, placed under conditions suitable for the emission of light, absorbs a portion of oxygen, which is replaced by an equal volume of carbonic acid.

6thly. This same substance, when deprived of its faculty of glowing, and then placed in contact with oxygen gas, no longer absorbs oxygen, or produces carbonic acid.

7thly. Oxygen mixed with either hydrogen or carbonic acid, in the proportion of 1 to 9, forms a medium in which the phosphorescence continues for several hours. We may, therefore, conclude, that it is in consequence of some alteration in the phosphorescent matter, that this ceases to glow after it has been for some days in pure oxygen, one portion only of which has been replaced by carbonic acid.

MM. examined the hydrogen in which he had kept several glow-worms for the space of twenty-four hours, and in which they had glowed for a few minutes only. The following is the result obtained when the gas was pure, and the experiment was conducted over mercury. The bell-glass being carefully filled by inverting it two or three times, in order to get rid of all the air adhering to the glow-worms, the volume of the gas was slightly augmented: with 8 cubic centimetres of hydrogen, he obtained an excess of 0·2 cubic centim. which was absorbed by potash. Thus, then, the insects produce carbonic acid, which must either be formed by the union of carbon with the oxygen remaining in the tracheæ, or exist ready formed in the animals. When the luminous segments alone are placed in hydrogen, with due precaution, they glow for a few seconds only and the gas is not altered.

8thly. Heat, within certain limits, augments the light of the phosphorescent matter; cold has a reverse effect.

9thly. When the heat is too strong, the phosphorescent matter becomes altered, as it also does when placed in the air or in any gas whatever for a certain length of time. This undoubtedly is the reason why these insects cannot live in all climates, and why they shine only during certain months of the year.

10*thly*. The phosphorescent matter, when thus altered, is no longer capable of emitting light or of becoming luminous.

He concludes also from other experiments, that the matter of phosphorescence prepared by the animal is preserved sometimes luminous, although the animal be deprived of life; proving that the life is not an indispensable condition of phosphorescence. By life, this substance is continually preserved with its properties entire, by the same process of nutrition which operates equally upon all parts of the animal.

Professor Matteucci examined the structure of the luminous organ by means of the microscope. On the removal of the luminous segments of the dorsal and abdominal membranes, there was perceived a yellowish, granular, globuliform matter, in which appeared groups of red globules, a great number of ramifications, and, moreover, a species of tubes which had the appearance of muscular fibre, but which, when closely examined, appeared hollow.

By looking at them at night, the light was seen to be emitted by the granular yellow matter; and when this was compressed between two glasses, the light was always observed on the edges of the examined portion. The abdominal membrane examined alone, after it has been washed several times in water in order to remove from it all the phosphorescent matter, is transparent, and furnished with a great number of hairs. The dorsal membrane, less transparent than the former, is likewise hairy, but it is also supplied, on its internal surface, with many tubes or tracheæ which penetrate the phosphorescent matter. He further adds, that he never separated the abdomen of a glow-worm without finding under the last luminous ring but one, a bright red vesicle, which, viewed by the microscope, is found to be made up of a group of red globules. He has never met with this vesicle in other insects of the same genus, and no work on comparative anatomy mentions it. In his ignorance of this science, he contents himself with announcing to zoologists the presence of this body.

As to the chemical nature of the phosphorescent matter, the substance, taken from the living animal, has a remarkable odour, resembling that of the sweat of the feet. It is neither acid nor alkaline; it dries readily in the air, appears to coagulate on contact with acids diluted with water; does not perceptibly dissolve either in alcohol, or ether, or in weak alkaline solutions. It dissolves and becomes changed in hot concentrated hydrochloric and sulphuric acids. By the employment of the last-mentioned acid, the solution does not become blue, which fact excludes the idea of the presence of albumen. Heated in a tube, it gives out the usual ammoniacal products. It does not present any obvious trace of phosphorus; of this fact he assured himself, by calcining this matter several times with nitre in a platinum crucible, and by treating the dissolved residue with the tests which indicate the presence of the phosphates. From all that has been now stated, we can no longer regard the presence of phosphorus as the cause of the light in these insects. Perhaps by operating on a large number, we might succeed in discovering a slight trace of phosphorus, which is usually found in all organised substances.

From all these experiments, Prof. M. concludes that carbonic acid is produced by the contact with oxygen of the phosphorescent matter alone, separated from the rest of the animal; that the light ceases to be produced when this gas is not present, and that by the contact of the latter, light and a volume of carbonic acid, equal to that of the oxygen consumed are produced; and that the phosphorescent substance of this insect, when not luminous, does not act on oxygen.

It is, therefore, natural to suppose, that in the luminous segments of these animals, enveloped by transparent membranes, and by means of the numerous tracheæ discovered here and there in these animals, atmospheric oxygen is brought in contact with a substance, *sui generis*, principally composed of carbon, hydrogen, oxygen, and azote. The presence of a great number of dispersed blood-globules intermixed with the granular luminous matter, proves that these segments are the centre of a peculiar organ of secretion; and he believes that this red vesicle, which he describes as existing above the luminous segments, merits the attention of naturalists. Excitation and heat affect the phosphorescence, as they do all other phenomena of the animal economy, by directly favouring combustion; and in this way we account for the effects produced by some agents on the phosphorescent substance alone, when separated from the animal. The example of an organic substance which burns in the air by absorbing oxygen and emitting carbonic acid, is not new; this is the case with decaying wood with oiled cotton, with very finely divided charcoal, and many other substances liable to spontaneous combustion. If, in the case which now engrosses our attention, the heat which ought to accompany the chemical combination be wanting, it may be easily explained. The quantity of carbonic acid disengaged from the luminous segments of each of these insects in a given time, is so small that the heat developed cannot accumulate there. The phosphorescence of wood, now referred to, as well as many other cases in which an emission of light accompanies chemical combinations, but which he need not farther notice, clearly prove that a disengagement of light may take place without any perceptible augmentation of temperature. Heat requires to be accumulated in order that its presence may be discovered by our instruments; and it is in this way we explain our inability to detect heat in the animals termed cold-blooded.

On the Influence exerted by certain Alimentary Principles on the Proportion of Fatty Matters contained in the Blood. By M. BOUSSINGAULT. Annales de Chemie et de Physique. 1848. T. xxiv. p. 460.—The milky appearance which the serum of the blood is observed occasionally to present, is known to depend on the presence of oil or of fat in suspension. The relations, either physiological or pathological, of the presence of fat in the blood are not very accurately known. But various physiologists have asserted, that it is possible to produce this lactescent state of the serum by feeding animals on fat. The same result might take place by subjecting individuals of the human race to an abundant regimen which may induce fattening.

Little doubt can be entertained that it is quite possible to induce great deposition of fat by certain modes of feeding. That is sufficiently well known to those who fatten cattle by giving them linseed cake.

M.M. Sandras and Bouchardat infer from their researches that the species of food exercises not any well-marked influence on the amount of the fat principles in the blood. M. Boussingault has made certain researches, the results furnished by which make him agree in thinking, with the physiologists now mentioned, that the blood of animals fed on aliments void of fat contains quite as much fat matter as the blood of animals which consume food greatly abounding in fat. He does not, however, agree with these inquirers, in ascribing the origin of this fat of the blood to small proportions of oily matters which previously were contained in the articles used as food, as the animals were fed on starch and white of egg, substances nearly destitute of fat. He thinks, further, that there is evidence that the fat of the blood does not always proceed immediately from fat or oily matter in the aliment. This proof is, that in the blood of animals kept several days without food, we find as large a proportion of fat principles as in the blood of an animal which has eaten lard, or liver, or nuts.

The animals employed were pigeons and ducks. The articles of food used were those already mentioned. It appears that in all instances the amount of fat was increased above the normal proportion, in rates varying from $\frac{14}{1000}$ to $\frac{40}{1000}$ parts of one grain in the instance of pigeons, and in a proportion of $\frac{182}{1000}$ to $\frac{188}{1000}$ in the instance of ducks.

These results show that more depends on the nature of the animal than on that of the food.

II. PATHOLOGY.

On the Etiology of Intermittent and Remittent Fevers. By JAMES F. GAYLEY, M. D., of Philadelphia. (American Journal of Medical Science, No. xxxiii. New Series. January 1849, p. 53.)—In this paper, Dr Gayley, is like Dr Mitchell, opposed to the hypothesis of marsh poison or malaria being the cause of ague and remittent fevers. He rejects the hypothesis of vegetable decomposition, and every other mode in which the formation of the alleged marsh poison has been explained. His idea is, that these distempers are mainly produced by high atmospheric temperature, operating during the day, at once on the surface of the earth and the persons of its inhabitants, and concurring along with the corresponding and alternating low temperature during night and early morning, in inducing first an excited and over-stimulated state of the human body, and then a greatly depressed and enfeebled one.

By the operation of such agents, he argues that the circulation of the skin and the various internal organs is disordered; the mutual balance in their circulation is subverted; the blood congested and accumulated towards the internal organs, especially the liver, acts as an irritant, instead of affording the means of nutrition; and the result is, first bilious or hepatic and gastric disorder, and then fever, which he seems to argue is a sort of effort to remove the bad effects of the previous hepatic and general visceral disorder.

The paper is lengthened and elaborate; and contains many illustrations of different degrees of force. The principal points are contained in the following summary.

It has been shown that the circulation is more active in the portal system in summer than in winter; also that it is subject to periodic augmentations every day after each meal. It has been also shown how a high temperature, long continued, produces debility of the liver, and how this debility is increased by the congestions produced by repeated exposures to the night air, which, in miasmatic districts, is always cool and damp. Under the continued action of these agents this debility goes on increasing: the tonicity of the organ decreasing in the same proportion, until at last, at one of the periodic determinations of blood to the part, the tonicity gives way, and the blood collects in abnormal quantities in the portal vessels, and gives us the phenomena of a chill. A chill is, therefore, nothing more than the natural periodic determination of blood to the part, taking place in excess. The time at which it most generally occurs corroborates this view, viz.—in the morning, about noon, or in the afternoon. This is the rule: but as the human body is exposed to a number of deranging influences, and people differ much in their habits, so we every now and then meet with cases which exhibit a deviation from it. We thus account for the occurrence of the first paroxysm. A recurrence of the chill is prevented by the fever that follows, so long as it lasts. But it having passed off, and the atony of the liver and portal vessels remaining, we might expect a return of the ague fit at any of the regular periods of determination of blood to the part. Here, however, comes another law of the disease which we are unable to explain. We cannot tell why a paroxysm occurring in the morning has a disposition to return the following morning, and thus give us a *quotidian;* while one occurring about noon is apt to pass over a whole day, and reappear on the third at the same hour, giving us a *tertian;* while one occurring in the afternoon will pass over *two* days, and thus give us a *quartan.* A solution of this must be sought for in the physiological laws of the organ involved. Our knowledge of these, however, is very imperfect; and so long as it is so, any attempt to explain its pathological laws must end in failure. The one is only a modification of the other.

If the congestion produced by the first paroxysm has been sufficiently great to excite inflammation, the febrile condition of system continues; but if the inflammation be not extensive, the organ still acts, to a certain extent, in accordance with its physiological laws; and the disease presents remissions and exacerbations corresponding somewhat to the paroxysm and pyrexia of the intermittent. We have thus the phenomena to which the term *remittent* is applied. But if the lesion produced by the congestion be very extensive, the derangement of system is so great as to set aside entirely the physiological laws of the organ involved; and we have the *continued* form of the fever.

In conclusion, the following, as a summary of his views on this subject, is given.

1. That the lungs and the liver are the great decarbonizing organs of the body; that, their function being complementary, the activity of that function is always in an inverse ratio.

2. That during winter, the lungs, from the part they perform in the generation of animal heat, are the more active organ. On the other hand, in summer, the liver is the more active.

3. That exposure to a low temperature repels the blood from the surface to the internal organs. If this exposure takes place in winter, the lungs being then the active organ, the brunt of the congestion falls on some part of the respiratory apparatus; hence, we have bronchitis, pleurisy, pneumonia, and catarrh, as the prevailing diseases.

4. But if this exposure takes place in summer, the liver being then in a state of stimulation, the force of the congestion falls on it. It becomes deranged, involving all those organs, more or less, whose blood has to pass through the liver to reach the heart. Hence, we have bilious affections, as intermittents, remittents, dysenteries, &c., as the prevailing diseases.

5. That the pulmonary diseases of winter and of cold climates, and the hepatic diseases of summer and of warm climates, are both produced by the same agents acting on the system. The different effects being solely owing to the different modifications of the agents, and the different conditions of the system when exposed to their influence: and we might with as much truth say that malaria was the cause of the one as of the other.

Nature is simple in all her operations. It is only in proportion to our ignorance that she appears mysterious. The mystery that all admit to hang over the origin of these diseases, and the failure that has hitherto attended every effort to explain it satisfactorily, we think is solely owing to the spirit of inquiry being on the wrong track. The idea of a specific poison, first started by Lancisi, has been embraced by most writers on the subject ever since: as a consequence, their labours have been expended in tracing the origin of a thing, the very existence of which is hypothetical.

Of the truth of this, the Proteus-like appearance that this agent assumes in the hands, or rather the heads of different writers on the subject, is *primâ facie* evidence. Thus one describes it as being softened or entirely disarmed by passing over 800 yards of water. (Sir G. Blane.)

Another thinks it can safely perform the voyage from Holland to England, yea, even to Scotland, a distance of not less than four hundred miles. (M'Culloch.) It cannot ascend to the second storey of a house, and yet can seize its victim on a mountain side four hundred feet high. (Ferguson). Stygian like, it ascends from the bowels of the earth, and angel-like descends with the dews from heaven.

Observations on certain Pathological Conditions of the Blood and Urine, in Gout, Rheumatism, and Bright's Disease. By ALFRED. B. GARROD, M.D, Assistant-Physician to University College Hospital. (Medico-Chirurgical Transactions, vol. xxxi, p. 81. London, 1848).

In this paper, Dr Garrod makes known some researches on the blood and urine, which tend to throw light on the pathological characters and relations of gout, rheumatism, and granular disease of the kidney.

First, Dr Garrod states that after analysing several times healthy blood, we should say blood of persons in apparent health, yet not with the perfect performance of excretion, he has always succeeded in detecting the presence of uric acid, even when the individuals were in the most perfect health, and when the blood was drawn only for the purpose of experiment. He has also found urea in perfectly healthy human blood. In one case, 1500 grains of serum gave of nitrate of urea 0·015 parts of one grain.

No trace of uric acid was found in 1000 grains of serum of the blood of the sheep, in which animal uric acid forms no part of the urinary secretion. Neither was any uric acid found in 1000 grains of serum from the blood of the pigeon, although the urine of these fowls, like that of BIRDS in general, consists entirely of urate of ammonia.

In the blood of the human subject, when the function of excretion is performed in its greatest perfection, no trace of uric acid can be detected, although, as is the case with BIRDS, the amount of uric acid formed within the system is very large. It is for this reason that we think Dr Garrod should have modified the statement regarding healthy blood as above proposed. The patient may enjoy health, yet the blood is not normal.

Secondly, Dr Garrod finds that the blood in gout contains uric acid in the form of urate of soda, and that this salt can be obtained in a crystalline state from the blood of gouty persons.

Thirdly, The proportion of uric acid is diminished in the urine immediately before the appearance of the gouty paroxysm.

Fourthly, In patients subject to chronic gout, with tophaceous deposits, uric acid is always present in the blood, and deficient in the urine, both absolutely and relatively to the other organic matters; and the chalk-like deposits, appear to depend on an action within and around the joints, vicarious of the uric acid excretory function of the kidneys.

From experiments performed by Dr Garrod on a person labouring under chronic gout, it results that the other organic solids are not very deficient; for, while the amount of urine was from 40 to 45 ounces in 24 hours, its specific gravity was 10:4, and it contained, in 1000 parts, 31·5 of solids, 12·5 of urea, 13·5 of other organic compounds, and 5·5 of fixed salts,

Dr Garrod infers, therefore, that the kidneys almost lose their power of excreting uric acid, but not the other solids of the urine.

Uric acid is nevertheless frequently thrown out of the blood by another process, giving rise to the formation of chalk-stones or gouty concretions. These always contain uric acid; and, when first effused, they appear in the form of a milky fluid, from which the watery portion is afterwards removed by absorption or some similar process.

Wollaston ascertained that these deposits consist chiefly of urate of soda. Lehmann obtained from the analysis of a deposit in the metacarpus.

	In 100 Parts.
Urate of soda,	52·12
Urate of lime,	1·25
Fixed salts,	14·16
Cellular tissue, water, &c.,	32·47
	100·00

Lehmann found in such deposits innumerable four-sided prisms of urate of soda. But although the analysis of these deposits gives fixed salts and other matters, yet, when first effused, they consist only of a clear fluid, containing multitudes of crystals of urate of soda; and it is to the reflections from the surfaces of these crystals that the opacity of the fluid is owing. The phosphate of lime found in large quantities in some gouty concretions, is probably due to a subsequent deposition from the occurrence of common inflammation around the parts. Dr Garrod, who has examined many of these concretions from different parts of the body, finds that all of them consist of bundles of crystals of urate of soda, united into masses. The size of these crystals varies much, depending apparently on the degree of rapidity with which the effusion takes place.

Fifthly, The blood in gout sometimes yields a small portion of urea; no albumen being present in the urine.

The very small proportion in which urea exists in the blood in gout may account, Dr Garrod thinks, for the circumstance of its frequently escaping detection. In one case, 1000 grains of serum gave of nitrate of urea only 0·03 of one grain, that is, three parts in one hundred thousand. He thinks, nevertheless, that the retention of this article in small quantities may explain one of the symptoms of gouty inflammation, namely, the presence of slight oedema; because in granular disease of the kidney, in which the blood contains a large amount of urea, this is a symptom so constantly produced.

Sixthly, The blood in acute rheumatism contains no more uric acid than it does in the state of health. In 1000 grains of serum of rheumatic blood, no urea can be detected. In three male patients in acute rheumatism, 1000 grains of serum gave traces of uric acid too small to be weighed. In a young woman with acute rheumatism, who had for some time been kept on low diet, 1000 grains of serum gave no indication either of uric acid or of urea. In one male patient with acute rheumatism, 1000 grains of serum gave numerous crystals of uric acid, but they could not be collected for weighing. In the same patient, 1000 grains of serum furnished no trace of urea.

In acute rheumatism, there is no evidence that the uric acid secreting power of the kidneys is deficient; and the urine is generally loaded with urea.

Seventhly, Uric acid is always present in the blood in *albuminuria*. The amount, however, greatly varies. When the functions of the kidneys are much impaired, it exists in quantities almost as great as in gout; in other instances its amount is small, but it usually exceeds that formed in ordinary (normal) blood.

Urea always exists in large quantities in the blood of patients labouring under *albuminuria*, a fact which has been proved for many years; and no relation is found between the amounts of urea and uric acid.

The kidneys are always, in *albuminuria*, impaired in their power of throwing off urea, but with regard to the uric acid, their power may or may not be impaired.

In a young girl of nine with albuminuria after scarlet fever, 1000 grains of serum gave of uric acid, 0·027 part of one grain. This is rather less than was obtained from the serum of one adult labouring under granular kidney in different stages, 0·037, but more than that obtained from the serum of another adult, 0·012 part of one grain. In this young girl the blood contained a large amount of urea. The urine was very albuminous, and the anasarca considerable.

During the evaporation of the serum of the blood in albuminuria, a peculiar odour of urine was frequently observed. This was not perceptible in the evaporation of healthy serum, or in that taken from gouty or rheumatic patients. Some of the colouring matter of the urine seemed to be thrown down with the uric acid in all cases.

From the results of these experiments, on the condition of the blood and urine, Dr Garrod infers, that uric acid is not a product of the action of the kidneys, as is frequently supposed, but that it is merely excreted from the system by these organs. They appear also to show, that the excreting function of the kidneys is not, as to the solid portion of the urine, simple, but that the urea and uric acid are separately eliminated; and that one of these processes may be impaired or destroyed, while the other remains entire.

It appears also to be a probable inference, that as in albuminuria, in which the urea-excreting function chiefly is impaired, a vicarious discharge of this body takes place in the dropsical effusions; so in gout in which the uric acid excreting function is defective, the chalk-like deposits are produced by a similar vicarious discharge of urate of soda.

Gout, the author further infers, to depend on a loss of power temporary or permanent, of the uric acid excreting function of the kidneys; the premonitory symptoms, and those which constitute the paroxysm, arising from excess of this matter in the blood, and from the effort to expel the *materies morbi* from the system. Any undue formation of this compound would favour the development of the disease; hence the connection between gout and uric acid, gravel and calculi; and hence the influence of full living, the use of wine, porter, and similar articles, and the want of exercise in inducing gout.

This hypothesis, the author suggests, may farther explain two facts which have been regarded as at variance with the humoral pathology of the disease; namely, its hereditary nature, and its frequent occurrence in what are denominated low states of the system. The peculiarity of the kidney, with reference to its function of excreting uric acid, may be transmitted; and when the function is permanently injured, it may not require an excessive formation of uric acid to cause its accumulation in the blood.

This view appears to be favoured by the observations of Dr Todd on the gouty kidney. This hypothesis further removes gout from the category, embracing Acute Rheumatism, since in this disease no excess of uric acid is found in the blood. These two diseases are analogous only so far as they affect similar structures.

III. SURGERY.

Statistical results illustrating the results following the Operation of Ligature of the Femoral Artery. By George W. Norris, M. D., Surgeon to the Pennsylvania Hospital. (American Journal of the Medical Sciences, No. xxxvi. October 1849.)

Dr Norris has collected from all different authentic sources 204 instances in which a ligature was applied to the femoral artery, chiefly for the cure of aneurisms, and has arranged them in a table, indicating the sex and age, of the patient, the disease or injury for which the ligature was applied, with the termination and other circumstances important to be known; and, from the facts thus furnished, he deduces the following conclusions.

Mortality.—The Table contains a list of two hundred and four cases in which the femoral artery has been enclosed in ligature by the Hunterian method. Of these, one hundred and fifty-four recovered, and fifty died. Six of the patients who recovered underwent amputation, in consequence of gangrene of the limbs.

Disease or Injury.—Of the whole number of cases contained in the tables, the artery was tied in one hundred and fifty-five, for the cure of popliteal aneurisms; in twenty-two, for femoral aneurisms; in six, for aneurismal tumours of the leg; in four, for pulsating tumours of the head of the tibia or condyle of the femur; in five, for varicose aneurisms; in three, for rupture of the artery, accompanying fractures of the leg or thigh; in eight, in order either to prevent or abate inflammation after wounds and dislocations, or to arrest hemorrhage, either primary or secondary; and in one, for a supposed popliteal aneurism.

Of the one hundred and eighty-eight cases in which the operation was performed for the cure of aneurisms, one hundred and forty-two recovered, and forty-six died.

Sex.—Of one hundred and eighty-three cases of aneurisms, in which this is noted, one hundred and seventy-seven occurred in males, and six in females.

Right or Left Side.—Of one hundred and twenty.four cases of aneurisms where the side is mentioned, sixty-three were on the right, and sixty-one on the left side.

Age.—The age is noted in one hundred and sixty-four cases of aneurisms, and of these there were:—

Under 10 years	1	a true aneurism.
Between 10 and 20 ...	4	
„ 20 and 30 ...	30	
„ 30 and 40 ...	72	
„ 40 and 50 ...	40	
„ 50 and 60 ...	14	
60, and above it, ...	3	

Period at which the Ligature separated.—In one hundred and thirty-seven cases in which the ligature separated spontaneously, it came away in ninety-one before the twentieth day; in thirty-seven, between the twentieth and thirtieth days; in fourteen, between the thirtieth and fortieth days; and in in five, beyond the fortieth day. The longest period to which it remained was the sixtieth day, and the shortest the fifth day.

Return of Pulsation in the Aneurismal Tumour after the application of the Ligature.—This was observed in nineteen cases. In No. 23, slight pulsation in the sac was observed on the day following the operation, on the next day it was more distinct, but two days afterwards it became much less. The patient recovered. In No. 25, some pulsation was observed in the tumour on the fourth day after the operation, and the patient died on the twelfth day of hemorrhage. In the third case, No. 60, pulsation-appeared in the tumour on the day after the operation, but ceased on the sixteenth day, the patient recovering. In No. 67, nine months after recovery from the operation, the tumour, which had diminished so much as to have become barely perceptible, and was without pulsation, reappeared, and pulsation in it became distinct. The cure was finally completed by compression.

In the fifth case, No. 78, the artery was enclosed in a ligature, on the 27th April 1821. On the 20th of July 1825, he was re-admitted, and stated that the tumour had entirely disappeared soon after he was discharged in 1821, but that, five or six weeks before his re admission, he observed that its swelling had returned at the upper part of the ham, and was at that time nearly of the size of a hen's egg. It was proposed to take up the artery between the part tied in the former operation, and the aneurismal sac; but as the first operation had not succeeded, the patient preferred amputation. He died eight hours after performance of the operation. The femoral was found to be obliterated, for the space of half an inch, at the part where the ligature had been applied four years before. Immediately below it, two small branches were observed to enter the continued trunk of that vessel. These were equal to half the diameter of the femoral vessel. The anastomising branches given off above the obliterated portion were a good deal enlarged.

In No. 95, all pulsation in the tumour ceased upon tightening the ligature, but returned in a few minutes. In half an hour after the operation it was nearly distinct as before the artery was tied, but on the third day the pulsation ceased. The patient died; and on dissection, the femoral artery, just below the part where the profunda was given off, was found to be divided into two nearly equal branches. These ran down parallel to each other, to the part where the artery passes through the tendon of the triceps muscle, when they again united. The ligature had been placed on the most superficial vessel.

In the eighth case, No. 96, which was one of varicose aneurism at the middle of the thigh, with an aneurismal varix near the ankle, pulsation was observed in the varix at the ankle on the fourth day; the patient recovered, but was not cured. In the year after the operation, the tumour was stationary with the same pulsation. In No. 102, aneurismal tumours existed in both the anterior tibial and popliteal arteries. Pulsation returned in both on the twelfth day, but ceased four days afterwards, and the patient was cured.

In No. 110, the operation was done in March 1829; pulsation ceased after its performance, and the tumour diminished in size. He returned to work, and it ultimately entirely disappeared. In the month of September following, the tumour reappeared with pulsation. By the application of a hard compress on the inner part of the thigh, which he wore for two months, night and day, the pulsation ceased, and one year after the operation no appearance of swelling was to be perceived.

In No. 134, pulsation in the sac returned the day after the operation, but rapidly disappeared. In No. 136, the disease was a varicose aneurism of the femoral. Four hours after the operation, feeble pulsation was noticed in the tumour, and on the following day became stronger. The patient died. In No. 144, the operation was done on the 24th of July; on the 31st, indistinct pulsation was observed in the tumour. By the 7th of August it had increased, and is afterwards noticed as present on the 15th and 25th. By the 4th of September it was free from all pulsation. In the fourteenth case, No. 155, pulsation was noticed in the tumour five months after the operation, though it was small and hard; the sac afterwards suppurated, and the case did well. In No. 161, pulsation reappeared in the sac on the tenth day, and gradually increased in violence until the man began to sink. In No. 190, pulsation was noticed in the tumour on the eighth day, and on the twentieth was no longer perceptible. In the seventeenth case, No. 191, where the operation was performed in March, the pulsation and size of the tumour had returned to nearly its original size and force by the middle of August. Compression on the artery was employed, and at the end of five days removed. After a fortnight, a slight thrill being again detected in it, it was reapplied for thirty-six hours, and a perfect cure effected. In No. 199, the patient had been operated upon, on the same side, six months previously, by Mr Greaves of Manchester. Pulsation returned in the sac twelve hours after it, and the disease was not cured. Upon close examination in the course of the cicatrix, the pulsation of a vessel was discovered, though certainly not of a natural sized artery, nor did any degree of pressure upon this command the pulsations of the aneurismal tumour. In the lower third of the thigh the pulsation of the artery was very perceptible, and pressure here immediately stopped the flow of blood into the tumour. Mr Cooper believed that the cause of failure of the operation which had been done, was owing, probably, to a high division of the femoral artery into two branches, and tied the vessel low down in the thigh. Immediately upon tightening the ligature, the pulsation in the tumour ceased, but in a few minutes became again as distinct as before the operation. A slight degree of compression was made over the sac On the following day the pulsation was less perceptible, and two days afterwards it had entirely ceased. During the ten following days the sac had become firm. In the nineteenth case, No. 200, the patient had been operated upon for a popliteal aneurism of the opposite side, three years previously, and was apparently cured, but when he presented himself for the second operation, a pulsating tumour of the size of a hen's egg was found in the ham. Upon examination of the femoral artery of this side, after the death of the patient, it was found to be obliterated for the space of two inches.

In addition to these cases, an instance is mentioned by Sir A. Cooper, where an operation for popliteal aneurism was performed by Mr Key, and the disease apparently cured, but a year afterwards the patient returned to Guy's Hospital with the aneurismal tumour nearly of its original size, and he amputated the limb.

Hemorrhage after the Operation occurred in twenty-four cases, of which twelve died and twelve were cured, one of the latter being with the loss of the limb. In No. 2, there was hemorrhage from the wound on the nineteenth and twentieth days after the operation, on the last occurrence of which the artery "was tied a little higher up." On the twenty-sixth day it recurred, and the patient died. In No. 7, death took place from hemorrhage on the eleventh day. In No. 23, the operation was done on the 14th of July. On the 24th there was slight hemorrhage from the wound, and on the 26th a return of it, which was arrested by compression. On the sixth of August, there was another bleeding, which was again arrested by pressure, and the case did well. In No. 25, bleeding had taken place on the eleventh day, and returned violently on the twelfth, and carried off the patient. In No. 29, hemorrhage took place on the forty-second day after the operation; it again occurred on the following morning, and an hour afterwards the man died. In No. 34, there was bleeding from the wound on the fifth day after the ligature separated, and the patient did well. In No. 111, there was hemorrhage on the fifteenth day, when the limb was amputated, and the patient recovered. In No. 38, the patient died from hemorrhage on the twenty-seventh day. In No. 40, there was bleeding on the twenty-third day; this recurred on the twenty-fourth, when the wound was enlarged and stuffed with lint. The patient was cured. In No. 56, an attempt to remove the ligature was made three days after the operation; two days subsequent to this, in consequence of loss of blood, a second ligature was placed higher up on the artery, and within a few days there was another gush, and the man died. In Nos. 64 and 74, death also took place from hemorrhage. In No. 68, there was hemorrhage from the wound on the sixteenth day, and a recurrence of it on the twenty-second, but the patient recovered. In No. 71, there was some hemorrhage eight days after the separation of the ligature, but it did not again recur, and the patient did well. In No. 92, a small non-pulsating tumour was observed, soon after the operation, about the middle of the wound, and the same night an alarming loss of blood from the part took place; the artery was now again secured higher up, and the patient recovered. In No. 100, there was hemorrhage to eight ounces on the nineteenth day, and no bad result followed. In No. 96, hemorrhage took place on the separation of the ligature, on the eleventh day, to the amount of a pint and a half, and the patient recovered. In No. 107, there was slight hemorrhage from the wound on the seventh day, and the patient afterwards died of gangrene of the limb. In No. 122, a cure took place, despite copious hemorrhages on the eighth and thirteenth days. In No. 136, bleeding to a large amount took place on the fifth day, and on the following one it recurred, and he died. In No. 129, there was slight hemorrhage on the twelfth day from around the ligature; this returned on the same evening, and a tumour was observed at the point of the ligature which pulsated, and had attained the size of an orange. On the following day hemorrhage again had taken place, when the artery was tied above the profunda, and the tumour on the thigh laid open. On the eighth day following this, there was another profuse bleeding, which was arrested by compresses and a bandage, and the patient recovered. In No 130, which was a femoral aneurism, a popliteal tumour of the size of an orange had existed in the same limb, in which a spontaneous cure had taken place after the formation of that in the thigh. The ligature was applied just below Poupart's ligament. On the eleventh day after the operation, hemorrhage to the amount of two pounds took place from the wound, which was stopped by compresses and a spica bandage. On the thirteenth, fifteenth, and seventeenth days, there were returns of the bleeding, but the patient recovered. In No. 193, the femoral was secured, July 7th, on account of a wound of the anterior tibial and interosseal arteries. On the 22d, there was hemorrhage to the extent of thirty-three ounces from the seat of operation, which was arrested by the application of cold and a tourniquet. On the 24th, in consequence of a renewal of it, the external iliac was tied. On the 26th, there was another alarming hemorrhage, when the saphena vein and a branch of the profunda were taken up. The patient died September 1. In No. 200, there was a slight hemorrhage previous to death on the nineteenth day.

Suppuration of the Sac occurred after the operation for aneurism in sixteen cases; and of these, six died and ten did well. In No. 3, it is stated, that the sac opened after the operation, but healed up like any other sore. In No. 19, on the fifth day after the operation, fluctuation became evident and the tumour was laid open. The patient died. In No. 7, fluctuation in the sac was noticed on the ninth day, and on the eleventh it burst, and the patient died of hemorrhage. In No. 26, at the end of a month, fluctuation was perceived in the tumour, and it was laid open. The patient died. In No. 31, the sac opened and discharged during the cure. In No. 38, an eschar formed on the tumour, after the operation, which discharged fetid matter. Warm milk and water was thrown into the sac, and on the same evening there was profuse hemorrhage from it. To arrest this, amputation of the limb was done, soon after which the patient died. In No. 52, the tumour sloughed, one month after the ligature, and the patient recovered. In No. 53, the operation was done in October 1813, and in the autumn of the following year the sac suppurated, was opened, and cicatrized. In No. 69, the tumour suppurated and was opened, on the fifteenth day after the operation, and the patient recovered. In No. 73, on the seventh day after the ligature of the vessel, the integuments covering the tumour were mortified, and an opening into it was made and its contents set free. The patient died. In No. 81, the skin over the sac, which was gangrenous at the time of the operation, sloughed on the fourth day. In No. 92, there was suppuration of the sac, which was opened and the patient cured. In No. 111, the tumour, which had been punctured before the artery was tied, afterwards suppurated and discharged. The man was cured after amputation. In No. 155, the sac suppurated more than five months after the operation, when an incision was made into it, and the patient cured. In No. 160, the tumour suppurated and discharged itself, and the patient recovered. In No. 181, heat of tumour was observed on the eighth day after the application of the ligature; three days after this fluctuation was evident, and an exit was given to the matter. The patient died.

Gangrene of the Limb.—This followed in thirty-one out of the two hundred and four cases contained in the tables, and it in no case occurred except where the operation was done for aneurisms. Of these thirty-one cases, Nos. 12, 19, 35, 58, 64, 83, 84, 89, 91, 94, 107, 108, 109, 128, 177, and 181, sunk at various periods after the operation. In No. 16, the last phalanges of the toes sloughed, as also a spot on the inferior part of the leg, and a portion of the fifth metatarsal bone exfoliated. The patient recovered. In No. 36, the operation was done on the 24th of July; mortification followed on the third day after it; amputation was done on the 16th of August, six days after which the patient died. In No. 85, the artery was tied on the 25th of January. On the 30th, symptoms of gangrene set in, and on the first of February the thigh was successfully amputated immediately below where the artery was tied. The gangrene in this case is stated to have been caused by the employment of fomentations of hot salt, notwithstanding strict injunctions to the contrary. In Nos. 97 and 98, the patient had aneurismal tumours in both hams; the left artery was first tied, and gangrene of the foot followed it. The ligature on the right limb was done at the request of the patient; gangrene occurred in it also, and death followed. In No. 99, mortification took place on the 9th day; the limb was ampuated on the thirty-fourth day, and the man died. In No. 114, the limb is stated to have been threatened with gangrene before the vessel was tied,—it followed the operation, and the man was cured after amputation. In No. 131, the ligature was applied on the 5th of September; profuse hemorrhage occurred on opening the sheath of the vessels. Gangrene immediately followed. Amputation above the knee was done on the 18th, and the patient recovered. In No. 145, amputation was done a week after the operation, in consequence of mortification, and was followed by death. In No. 148, the operation was followed by gangrene of the limb, and the thigh was removed unsuccessfully. In No. 182, gangrene appeared on the third day; amputation was done, after a line of demarcation was formed, and the patient recovered. In No. 183, the limb was amputated on the 22d day, and was followed by death in three hours. In No. 195, the tumour had burst previous to the operation, which was done on the 1st of March; gangrene of the foot and leg followed, and on the 8th the thigh was amputated, and the patient got well. In No. 198, symptoms of mortification appeared on the third day, after which trismus set in, and the man died. In No. 203, the operation was followed by gangrene of the feet and leg. The thigh was amputated during its progress, seven days after which death occurred from gangrene of the stump.

Cause of Death.—Of the two hundred and four cases contained in the tables, fifty died. Of these, twenty-three died from mortification of the limb; eight from hemorrhage; five from phlebitis; three from tetanus; two from hectic and diarrhœa; one from thoracic inflammation and abscess in the course of the artery; one from sloughing of the sac; one from the bursting of an aneurism of the aorta within the pericardium twelve weeks after the operation; one from fever; one from absorption of pus; and in four the cause is not noted.

Mistakes in Diagnosis.—In four of the cases included in the tables, mistakes in diagnosis occurred. In No. 83, the precise nature of the tumour could not be ascertained, and it was punctured before the operation. In No. 111, the tumour was mistaken for an abscess, and opened a week previous to the operation; after bleeding three pints it ceased spontaneously. In No. 138, the tumour was the size of a goose's egg, distinctly pulsated, and was said to have shown itself suddenly, while walking, three weeks previously. Compression was at first attempted, and afterwards the artery was tied, notwithstanding which the swelling continued to increase. On post-mortem examination, it was found to consist partly of a fibrous tissue divided into lobes, and in part of a soft substance containing cells filled with a serous fluid. The artery ran over the tumour between two of the sacs, and through these its pulsations had been so communicated as to give the sensation of the whole tumour pulsating. The right lung was converted into a brain-like mass. In No. 196, the patient was a black woman, in whom there existed great swelling of the whole limb, but particularly of the popliteal region. Suspecting deep suppuration, an incision, an inch in length, was made into it, from which nothing but a little serum, slightly coloured with blood, escaped. The integuments in the popliteal region afterwards ulcerated, and some days subsequently there was a profuse hemorrhage from the part when the artery was tied.

Two instructive cases of mistake in the diagnosis of popliteal tumours have been published by Mr Laurence, of London. The first (*Medico-Chirurgical Transactions*, vol. viii. p 497) was that of a large aneurism filling up the whole ham, and extending on both sides of the femur towards the front of the limb. It had begun behind, and had existed for five months; had a firm fleshy feel, being a little softer at one of its anterior protuberances than in other parts; was not tender on handling, but gave the patient great pain. The surgeons of St Bartholomew's, in consultation, viewing it as a large and rapidly increasing tumour, recommended amputation, which Mr L. performed, having first plunged a lancet into the softest part of the swelling, to the whole depth of the blade, without giving issue to any fluid. Examination of the amputated limb showed the tumour to be a popliteal aneurism, containing an immense mass of firm bloody coagulum; and, although the sac had been freely penetrated by the abscess lancet, no part of its contents escaped. The eminent surgeon who records this adds, that he "has stated the case to put others on their guard; and shall be happy if what he has said should, in any instance, prevent so serious a mutilation as that which his patient suffered."

The second case was that of a man, aged thirty, with a swollen and painful state of the right knee joint, consequent upon a fall. Shortly after, a swelling arose immediately above the knee, and gradually extended around the back part and sides of the thigh. Near the tendon of the triceps a softening of the swelling indicated the probability of its containing matter, and a small incision was made into it, from which about four ounces of arterial blood flowed. On examining the swelling more closely, pulsation in it was now discovered, and it was agreed that sufficient ground existed for believing the tumour to be a popliteal aneurism, and accordingly the femoral was tied. The pulsation immediately ceased, and its size gradually diminished; but, after some time, it again enlarged, became painful, the skin covering it sloughed, and the slough extended into the tumour, and the man shortly after died. The tumour was found to consist of a soft fibrous and dense osseous tissue,—the latter originating from the femur. The femoral and popliteal vessels were sound.

Difficulties of, and Accidents during the Operation.—In eleven of the two hundred and four ligations of the femoral artery, the crural vein was either included in the ligature, or wounded in the operation. In Nos. 2 and 90, the vein as well as the artery was tied. In Nos. 13, 73, 94, 107, and 159, the vein was wounded in the operations. In No. 74, the ligature was found to have passed through the vein. In No. 131, profuse hemorrhage occurred on opening the sheath of the vessels. In No. 146, the vein was wounded during the operation; the patient recovered, and lived four and a half years after it. On dissection, it was found to be obliterated to the extent of three inches. In No. 161, the vein was pricked in the operation, and a small portion of it included in the ligature along with the artery. It may be well here to mention, that to Sir A. Cooper one case occurred, where the patient placed himself upon the table, for the purpose of undergoing ligature of the femoral vessel for a popliteal aneurism, and died before the first incisions were concluded, in consequence of the bursting of an aneurism of the aorta.

In the Southern Journal of Medicine and Surgery, for May 1848, an interesting case of false aneurism in the thigh is related, in which an attempt was made to deligate the femoral artery at the crural arch. A ligature was passed around what was supposed, by all present, to be the artery, but on post-mortem examination it was found that it embraced a portion of a tendon, and that the femoral artery was not tied. The patient was so exhausted from previous hemorrhage, at the time of the operation, that no pulsation could be felt in either of the lower extremities.

Aneurisms in both Sides.—Of the cases of aneurism included in the tables, nine presented popliteal aneurisms on both sides. In Nos. 22 and 23, both aneurisms were observed when the patient presented himself to his surgeon. The left, at the time of the first operation, was small and increased rapidly after the ligature of the first artery. In Nos. 60 and 61, the aneurism of the left side did not exist at the time of the operation upon the right. In Nos. 79 and 80, the tumour on the right side showed itself but a few weeks before the second operation. In Nos. 97 and 98, the aneurisms were both large, and of seven months' standing; the right artery was tied two months after that of the left. In Nos. 125 and 126, the aneurisms occurred simultaneously, and had existed for fifteen years. In Nos. 129 and 130, there was incipient disease of the right ham at the date of the operation upon the left. In Nos. 167 and 168, the second tumour did not make its appearance till after the cure of the first. In Nos 188 and 189, the disease seemed to be beginning on the right side at the time of the first operation. In Nos. 201 and 202, the tumour in the left ham was noticed some time after that of the right side had been operated upon. No. 84 was also a subject of aneurism in each ham. In No. 197, the tumour in the ham was first observed about ten days after a ligature had been placed on the external iliac of the opposite side for the cure of a femoral aneurism. No. 20 had been the subject of popliteal aneurisms of both sides, and the left limb was amputated some time previous to the ligature of the vessel of the right side. In No. 200, also, the artery on the left side had been tied three years previously for a popliteal aneurism.

Pulsating Tumours of the Head of the Tibia, or Condyle of the Femur.—Four cases are included in the tables, in which the femoral artery was ligatured for the cure of these affections. They all recovered; but in two of them, although cures appeared to follow, there were afterwards returns of the disease. In No. 68, the limb was amputated between three and four years after the ligature had been applied, the tumour having attained a great size. In another, which occurred to the writer, although the ligature of the vessel was apparently followed by a cure of the disease, the tumour returned, and, eleven months after it, I amputated the limb. In three of the cases in which the vessel was tied for these tumours (Nos. 68, 151, and 187), pulsation reappeared; in the first on the sixteenth day, but soon ceased; in the second on the seventeenth day, and ceased on the twenty-sixth day; and in the third on the seventh day.

Varicose Aneurisms.—The tables given contain five cases of varicose aneurisms of the lower limb. Four of these proved fatal, and in one no benefit was received from the operation. In No. 64, where the disease was situated on the posterior tibial artery, death occurred from gangrene of the limb and hemorrhages. In No. 94, where the femoral was effected, the operation was soon followed by gangrene and death. In No. 96, the disease was situated in the middle of the thigh, and the ligature was placed as nearly as practicable to the sac. The case went on favourably till the separation of the ligature on the eleventh day, when the patient came near losing his life from hemorrhage, and was in no way benefited by the operation. The contents of the tumour never coagulated, nor did the tumour decrease, and in the following year it remained stationary, with as much pulsation as before. In No. 136, death soon followed the ligature of the artery, and, on dissection, the vessel was found to be enlarged nearly to the size of the aorta, with its coats so thin as to give it nearly the appearance of a vein. In No. 148, the artery was tied immediately below Poupart's ligament. It was followed by mortification of the limb, and the thigh was removed six inches below the hip joint. The disease afterwards returned, and secondary hemorrhages terminated life.

In connection with the above instances of varicose aneurism, in which the femoral artery has been tied, it may be interesting to contrast their results with those in which either the external iliac has been secured, or other treatment has been adopted, for the same affection. In the case of Baroni, where the disease was seated in the inguinal region, and the external iliac was taken up, an incision was made into the sac on the forty-fifth day, in consequence of repeated bleedings, with the view of securing all the vessels. The hemorrhage was so frightful, that the patient was only saved from dying on the table by making strong compression, and death occurred eleven days after. In Dr Fleischer's patient, where the affection occupied the middle of the thigh, and an incision into the sac was made, the fatal result took place from like causes. In the history reported by Hennen, where the external iliac was tied, the affection being seated in the upper third of the thigh, gangrene and death speedily followed; and in the case of Dr Morrison, ligature of the external iliac was also made, for a like affection seated at the same part, and death in a short time terminated the sufferings of the patient. In the instances reported by Lallemand, where the disease was seated above the middle of the thigh, and was of years' continuance, the femoral artery was first tied a little below Poupart's ligament. On the sixth day, in consequence of hemorrhage, the external iliac was ligatured, and on the same evening the bleeding again recurred, and he died. In a case recently published by Dr Mott, the disease occupied the upper part of the thigh, and had existed for two and a half years. A ligature was applied to the external iliac; gangrene supervened, and the man died on the sixth day.

Observation, however, shows that varicose aneurisms in the lower extremity, after a certain lapse of time, in several cases have become stationary; and instances are recorded in which the affection has continued for years, causing but little inconvenience. In 1820, a patient fell under the notice of M. Dupuytren, with a large varicose aneurism of the upper part of the thigh, which had continued for twelve years without injuring the health, or in any way interfering with the free use of the limb; and in the spring of 1835, a patient was in the wards of M. Velpeau, at La Charité, in whom a like affection, seated high up in the thigh, had existed for more than twenty years, without materially interfering with the use of his member. A few years since, I had an opportunity of examining, along with several other surgeons of this city, a case of long continuance, which inconvenienced, though it did not distress, the patient. In face of these facts it is, in my opinion, no longer a question whether or not operative measures should be resorted to in the lower limb in the treatment of such cases. Sound surgery condemns it. The affection does not necessarily compromise life, or the free use of the member; and resort to the knife should not be had so long as the infirmity can be made at all bearable by the use of compresses, laced bandages, and other like means. If in any case an operation for varicose aneurism in the lower extremity is absolutely demanded, all reasoning, as well as experience on the subject, goes to show that no good result can be expected from the application of a ligature by Hunter's method.

IV. MISCELLANEOUS.

Instance of Death under the Use of Chloroform. By M. Robert, Academie de Medecine. (Archives Generales de Medecine, 4ieme S. Tome xxi., Octobre 1849, p. 232.)—On the 11th September 1849 M. Robert communicated to the Academy of Medicine, in a note on the danger of Chloroform, an instance of fatal termination under the use of that agent which deserves to be recorded.

The object of M. Robert was, if not to resolve the question of the propriety of the administration of anæsthetic agents, at least to furnish some materials capable of determining, whether, in the impossibility in which we are placed of distinguishing *a priori* the individuals to whom chloroform is a fatal poison, it might not be possible to find in the varied symptoms to which it gives rise, some marks by which danger might be foreseen, and might thereby be prevented.

Throwing aside, says M. Robert, the instances in which insensibility is rapidly established, the phenomena may be divided into two distinct periods. In the first, which is the period of excitement, there is at the beginning injection of the conjunctivæ, then agitation, a little delirium, and some convulsive movements; sensibility appears extinct; but often it is not completely gone, or it is gone only for a very short time. If at this time we attempt to perform an operation, it is scarcely commenced, when the patient is thrown into violent movements, which he endeavours to avoid, and in which he would be exposed to most serious dangers if he were not strenuously restrained. If the use of the chloroform be continued, another series of symptoms takes place. The countenance becomes pale, the muscles are relaxed, and by little and little fall into a complete state of resolution. Respiration becomes stertorous; insensibility is so deep that the individuals may undergo the most severe operations, the most painful mutilations, without evincing any proof of the slightest sensibility. This is the second period; the period of collapse; or the surgical state.

When the symptoms of the first period present only a moderate degree of intensity, and a duration not very long, that is, only some minutes, chloroformization offers probably no danger, and the operator may by means of intermitting and repeated inhalations, when they are required, maintain the state of insensibility for a long time; for half an hour, three quarters of an hour, one hour even, according to the representations of some practitioners, without producing in the patients any unfavourable affection. But there are individuals in whom this period is characterised by symptoms of great intensity. Agitation is excessive; the breathing hurried; the patients must have been delirious, as the movements were so violent, that if they had not been restrained, the patients would have destroyed themselves. This period of excitement, when it has arrived at this degree, and is prolonged beyond a few instants, denotes great disorder in the nervous system. According to M. Robert, if the administration of the chloroform be not suspended, and if the operator endeavours to obtain the period of tranquillity and collapse, he will place the life of the patient in danger. Among one hundred and twenty operations performed under the influence of chloroform, this surgeon met with three individuals presenting in different degrees the circumstances now under consideration. Of the three instances which he records, one only may be noticed, because it is the only one in which death took place, and that in a very rapid manner.

A man aged forty-nine years, a dealer in horses, of herculean strength, addicted to the greatest excesses in drunkenness, had for eighteen years in the right groin a hernial tumour, which he stated he had always kept up. But three days previously to the time of his illness he fell from his horse. The truss was broken in the fall; and at the same time the hernial tumour came down of great size, and could not be replaced. When M. Robert arrived at the residence of the patient, the tumour had been strangled for two days and a half. The patient was vomiting abundantly matters of reddish colour, and evidently stercoraceous. The swelling in the groin, sufficiently large in size, was soft and without much pain. The abdomen was indolent.

In order to facilitate reduction by taxis, M. Robert had recourse to chloroform. With this view, he placed a small sponge within a compress rolled in the shape of a cone, and fixed with a needle. The administration of the chloroform he conducted himself, taking the precaution always to allow to the patient a free space between the apparatus and his face, that the entrance of air might not be impeded. For about ten minutes the patient experienced no remarkable sensations; but very soon the eyes appeared injected; he became talkative, raving like a drunken person; respiration became hurried; the skin was covered by a profuse sweat; lastly, agitation became so great, that three assistants were required to hold him. Sensibility, nevertheless, was not extinguished. As the agitation of the patient did not permit the performance of the taxis, and still less of the operation for reduction, M. Robert continued the inhalation of chloroform. After the space of half an hour, agitation was allayed; the muscles were relaxed, and the operator was then enabled to attempt the taxis. But as this was unsuccessful, at the end of a quarter of an hour, the operation was forthwith undertaken. It presented some peculiar difficulties, by which its duration was protracted. The hernial tumour was formed by the cæcum; and like hernial tumours of this bowel, it was irreducible. Consequently, after having divided the strictures in the inguinal canal, M. Robert required to open the intestine, in order more completely to ensure the course of the fæcal matters. During the operation, which lasted at least twenty minutes, the patient was agitated several times; but he was immediately put into the anæsthetic condition by means of inhalations of short duration. After the operation he recovered sensibility. The pulse was small, a little frequent; and a little delirium continued for about one hour. In the day he was tranquil enough. About one o'clock in the morning he requested some drink. Half an hour after, the attendant having approached his bed, found him lifeless. He had expired without noise, without agitation, without being observed even by the neighbouring patients.

The body inspected thirty hours after death, presented the following state of parts.

There was no rigidity. The neck and face were violet coloured, as if ecchymosed.

The brain and its membranes were greatly injected.

The lungs were pliant and crepitant, but in all parts loaded with dark-coloured blood.

The cavities of the right and left sides of the heart contained also a large quantity of black blood, half coagulated, and without any mixture of aeriform fluid.

The liver was gorged with blood. The spleen was equally distended. There was no trace of peritoneal inflammation.

The hernia was formed by the cæcum, and presented no sac. It was in its whole extent surrounded by loose filamentous cellular tissue. In turning it outwards, the intestine had performed a movement of half-rotation on its axis, in such a manner that the ileo-cæcal appendage being carried forwards, formed a sort of collar round the termination of the ileum. This was the only obstacle which caused a mechanical impediment to the movement of the fæcal matters; and this obstacle the operation had neither discovered nor removed.

This instance is given in detail, because it forms the foundation of the reasoning of M. Robert. According to M. Robert, chloroform was in this case the sole cause of death. The patient was destroyed neither by pure simple asphyxia, nor by asphyxia associated or superadded, as is said, to the action of chloroform, but certainly by the exclusive influence of the last agent. M. Robert concludes with the following inference. When inhalation of chloroform, instead of inducing sensibility in a tranquil and rapid manner, induces on the contrary an intense irritation in the brain, and causes agitation, delirium, and convulsive movements, danger is present. Prudence requires that the inhalation be immediately suspended, and that the operator refuse allowing to patients the benefits of anæsthesia, which may have a fatal termination.

In the discussion to which the communication of M. Robert gave rise, M. Velpeau stated that the deaths reported by M. Robert he did not regard as capable of being ascribed to the action of chloroform; and that instances of sudden deaths soon after operations had been long known in hospital practice, before anæsthetic agents were in use. Both he and M. Roux disapproved of the mode of administration.

It is impossible to deny in the case now given that the cause of death is doubtful. The patient might have died whether chloroform had been administered or not when the lesion present was so serious as an irreducible hernia unreduced for two days and a half; when an opening was made into the bowel; and when, after all, inspection showed that there was an internal cause of stricture of the bowel.

In the following case these causes of ambiguity are not present—(Casper's Wochenschrift, Medical Gazette, No. 1163, vol. xlv., p. 483, 15th March 1850.)

A young lady aged twenty years, in good health, well made, and of good constitution, required to have a tooth extracted. The dentist to whom she applied, having made three unsuccessful attempts, after putting the patient under the influence of chloroform at nine in the morning, the patient requested his attendance at her own residence about noon, in order that the tooth might be extracted. Two friends were present.

For the administration of the chloroform the dentist employed a piece of sponge, of a triangular shape, about three-quarters of an inch in length, and about the same in thickness. He states that he poured thereon from twelve to sixteen drops of chloroform, covered it with a napkin, and then held it under the patient's nose. In a few moments she became motionless. The dentist could not, however, now extract the tooth, because her jaws were firmly closed. While he was attempting to open her mouth she awoke; he then sprinkled some water in her face. Both the dentist and one of her friends endeavoured to persuade her to submit to the operation without the use of chloroform. But she experienced so much pain in the trial that she was still determined to have recourse to the use of chloroform. The dentist deposed that he again poured from twelve to sixteen drops of chloroform on the sponge, and applied it under her nose, keeping her mouth open with a piece of wood. She was yet only partially unconscious, and exclaimed, "I feel, I feel!" The dentist added four or five drops more. After two or three inspirations one of the friends deposed that she heard "rattling" in her throat, that her countenance became livid. The dentist, who was about to endeavour to remove the tooth, perceiving this change, immediately sent for medical aid.

The other friend, who witnessed the whole proceeding, gave a similar account, adding, that the dentist had desired the deceased to inspire the chloroform through the nostrils, and to expire it by the mouth. Scarcely had she done so, when she closed her eyes, and became senseless and motionless. The same witness deposed that immediately after the second application of the chloroform to the nostrils, the deceased suddenly stretched herself out, and that a yellow fluid flowed from her mouth, followed by a whitish froth in considerable quantity. The nose, forehead, and cheeks became of a blue colour. Dr Kops, a regimental surgeon, who saw her immediately, found her quite dead. On entering the room he perceived so strong a smell of chloroform that his head was unpleasantly affected thereby. He found the chloroform bottle open on the table. The dentist stated that he could not account for this circumstance, as he was always careful to stop the bottle after using it. This, moreover, he urged could not have contributed to the fatal result, because the patient had become suddenly comatose, immediately after the third application. The patient had died in an instant after the first convulsive twitch of the face.

The body was examined fifty hours after death.

There were no external lesions which could be attributed to the action of chloroform, but abundant evidence of incipient putrefaction. The membranes of the brain were slightly congested, and the larger veins contained some air. The substance of the brain was in its natural condition. The ventricles were empty. The base of the brain, the pons, and medulla, were healthy. The sinuses were not unusually full of blood.

The lungs presented nothing morbid; the bronchi contained a little bloody froth. The blood was of the colour and consistence of cherry-juice. The heart was soft, flabby, and collapsed. Its coronary vessels and cavities were empty, and presented the bluish-red discoloration of decomposition.

In the abdomen, the liver was of its usual colour and consistence, but exsanguine. The gall-bladder quite empty. The spleen rather large, and distended with the above described kind of blood. The other viscera were also normal.

Dr Casper observes that the results of the autopsy confirm the opinion derived from cases which have occurred in France, England, and elsewhere, that death is caused by a directly poisonous influence, exerted on the heart.

Dr Casper attributes death in the present case immediately to the administration of chloroform, but acquits the dentist of any imputation of carelessness in its exhibition.

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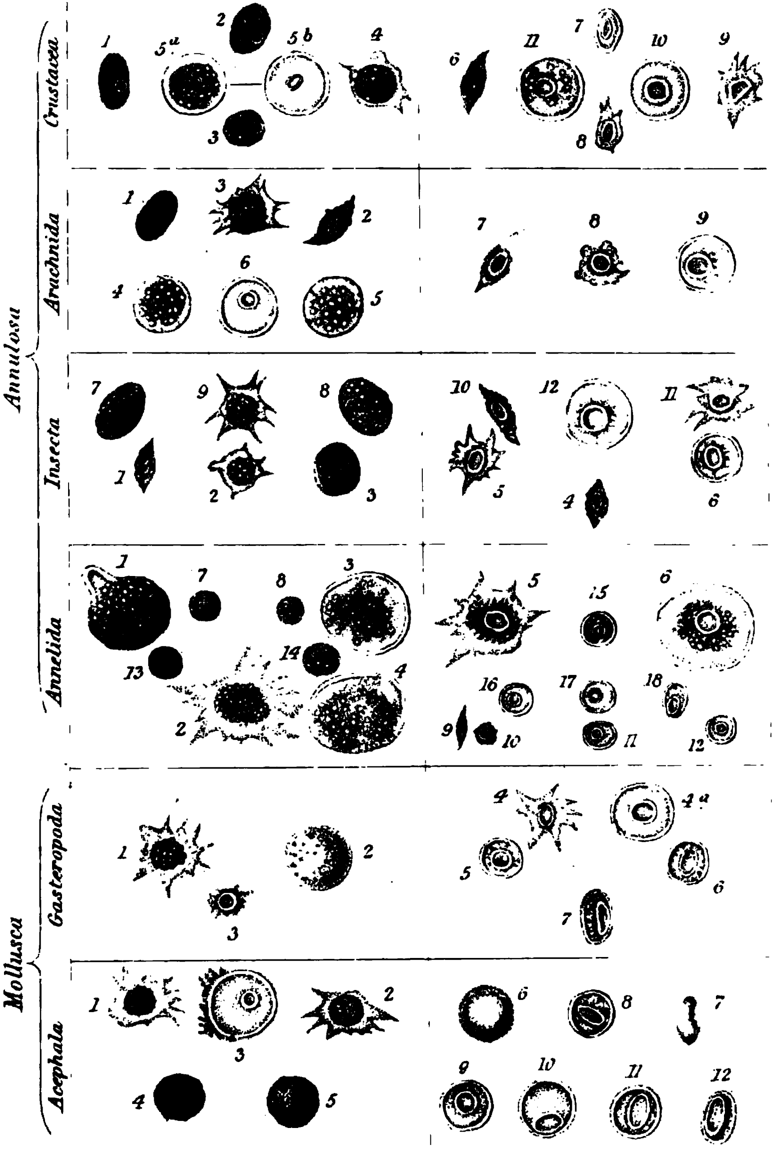
The Blood Corpuscle.



Wm Aikman Sc.

The Objects here represented are Magnified 550 Diameters.

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